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**Does Gentrification Displace Poor Households?  
An 'Identification-Via-Interaction' Approach**

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## **Abstract**

My theoretical model motivates an ‘identification-via-interaction’ (IvI) approach that separates the causal impact of gentrification on poor exits from endogenous channels. In the empirical analysis, I create a measure of gentrification as the increase in the share of neighbourhood residents who hold a university degree based on the UK Census for 1991, 2001 and 2011. Applying the IvI approach for a sample of private renters from the British Household Panel Survey, 1991–2008, I find that gentrification results in displacement. The IvI approach has general applications in estimating causal relationships where variables are highly endogenous.

Key words: neighbourhood change, mobility, turnover, causality, cities, urban, housing  
JEL:R21; R23; R31; C20

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# I. Introduction

Narrowly defined, gentrification is an influx of rich or middle-class residents into a previously poor or working-class urban neighbourhood. More broadly, though, gentrification may be considered the major urban trend of the 21st century.<sup>1</sup> As one would expect there is a significant literature examining its causes and consequences.<sup>2</sup> However, the literature has been unable to provide a definitive answer on one of the most fundamental issues: whether gentrification causes individual households to exit their housing units. This question of ‘displacement’ is important because of the potential for harm to households. The emotional response to being displaced has been documented as a grief similar to that experienced when losing a friend or close relative [Slater, 2009]. Furthermore, findings are emerging that mobility may be associated with powerful negative disruption effects for children in households [see e.g. Chetty et al., 2016, Gibbons et al., 2017]. The displacement question is also important in terms of policy design and for framing policy responses to gentrification. For example, if displacement occurs, then policies for mixed communities may benefit existing residents only if combined with greater measures to protect them from higher housing costs.<sup>3</sup>

Clearly, the displacement question is a causal one but, as yet, no causal answer has been provided. The usual empirical approach is to test for a significant increase in the probability that poor households exit their housing units if they live in gentrifying neighbourhoods. I find five such studies. Vigdor’s [2002] seminal study of Boston and Freeman and Braconi’s [2004] of New York are the first to compare exit rates for poor households between gentrifying and non-gentrifying neighbourhoods.<sup>4</sup> Three later studies, Freeman [2005], McKinnish et al. [2010], both for the US, and Freeman et al. [2016], for England and Wales, innovate by studying gentrification using fine-scale neighbourhoods nationwide. Notably, none of these studies finds evidence of displacement. A limitation to these findings is that the empirical approach relies on the identifying assumption that gentrification is exogenous to poor moves.

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<sup>1</sup>Following suburbanisation in the 20th century and urbanisation in the 19th century, roughly speaking.

<sup>2</sup>There is a large literature on the causes of gentrification and income segregation patterns, more generally [e.g. Buck, 2001, Brueckner and Rosenthal, 2009, Brueckner et al., 1999, Guerrieri et al., 2013, Glaeser et al., 2008, LeRoy and Sonstelie, 1983, O’Sullivan, 2005, Rosenthal, 2008, Tivadar, 2010, Wheaton, 1977]. There is also a large literature documenting gentrification and its impacts, for example on crime, housing costs, and jobs [e.g. Bostic and Martin, 2003, Guerrieri et al., 2013, Lees et al., 2013, Lester and Hartley, 2014, Meltzer and Ghorbani, 2017, Ellen and O’Regan, 2008, Glaeser, 2008, Helms, 2003, Vigdor, 2010]. It is also worth noting that some of the biggest concepts in urban economics, such as the creative class and the consumer city, have gentrification at their core [Glaeser et al., 2001, Florida, 2002].

<sup>3</sup>Examples are estate renewal programmes such as HOPE VI in the US and Housing Market Renewal in the UK.

<sup>4</sup>Earlier studies made no comparison to a control group of non-gentrifying neighbourhoods.

I contribute to the literature by highlighting how this assumption might be violated and by suggesting a credible identification strategy.

My conceptual model identifies two endogenous channels between gentrification and poor moves. The first channel reflects the fact that gentrifying neighbourhoods are not a random selection of neighbourhoods. According to Buck [2001], they are typically more central, with better transport access and older dwelling stock than other neighbourhoods. It is likely then, given the presence of neighbourhood sorting, that a certain type of household would be living in these neighbourhoods even before any gentrification occurred. Therefore, higher (or lower) observed exit rates in gentrifying neighbourhoods may simply reflect different mobility rates for the type of households who live in those neighbourhoods, rather than any effect of the gentrification. This first endogenous channel describes a selection problem. The second channel reflects the fact that (to some extent) gentrification may be the *result of* poor exits as opposed to *resulting in* poor exits. The model illustrates how that if the poor decide to leave the neighbourhood due to changes in their own preferences or changes in amenities that they care about then gentrification occurs as a result. This second channel describes a reverse causality problem. Given the presence of these two endogenous channels, gentrification cannot reasonably be considered exogenous implying that the usual approach may provide inconsistent estimates.

Notably, no studies have attempted to apply an instrumental variables (IV) approach, most likely due to the difficulty in finding something that impacts rich moves but not poor moves. To illustrate, consider using a local amenity shock as an instrument for gentrification. Whilst the amenity shock may predict gentrification (because the rich like the amenities), it may also directly lower poor exits (because the poor like the amenities too). In this example, violation of the exclusion restriction leads a negative bias that may conceal real displacement effects. In different examples, the bias may be positive.

I propose identification-via-interaction (IvI) as an approach that may be useful in such cases where there is inherent endogeneity but where valid instruments cannot easily be found. The approach involves interacting the endogenous ‘treatment’ with a moderating variable that indicates vulnerability to the treatment. Only if a treatment effect is present will its differential effect with the moderating variable be captured in the interaction term. The identifying assumption of this approach is that endogenous relationships are unaffected by the moderating variable. On an intuitive basis, the interaction acts as a filter that lets through only the causal part of a relationship and filters out the endogenous parts. My conceptual model motivates this approach in my case of gentrification and displacement.

Specifically, the model illustrates that the causal displacement channel is expected to be differential with a random budget-shifter, but that the endogenous channels of selection and reverse causality are not. The key intuition is that households will be less able to resist displacement pressure when their budgets are already stretched. Thus, if a higher poor exit rate under gentrification indeed reflects displacement, this difference will increase with a negative budget shift. However, if the higher poor exit rate instead reflects an endogenous channel, say, that the type of households living in gentrifying neighbourhoods is different, this difference is not expected to change with a budget shift. The model highlights some potential threats to the exclusion restriction that I examine empirically.

My empirical analysis applies the IvI approach to examine displacement of private renter households using the British Household Panel Survey (BHPS), 1991–2008.<sup>5</sup> Making use of the UK Census for 1991, 2001 and 2011, I create a neighbourhood index for gentrification as above-average intercensal increases in the share of households holding a university degree. I then interact this gentrification variable with a random budget-shifter. Specifically, I use ‘degree days’, a national-level year-to-year measure of cold weather that impacts on the household budget through heating fuel bills. My findings indicate the existence of displacement effects for low-income private renters. In fact, income interaction specifications indicate that displacement effects are present up to a household income per capita of around 1.5 times the average for the city and year. A non-linear specification indicates that displacement effects begin to bite at levels of gentrification that equate to a degree share increase of about 10 percentage points above the national average. The main result stands up to controls for potential threats to the exclusion restriction, performs well in various placebo tests, and is robust across a wide range of sensitivity tests. Heterogeneity analysis indicates that the displacement effect is fairly constant across households of different types, but that displacement occurs only for gentrification of working class and urban neighbourhoods.<sup>6</sup>

An important contribution of this paper is the conceptual framework. I combine residential mobility into an equilibrium-sorting framework of the [Epple and Sieg \[1999\]](#) type to help provide analytical clarity to the displacement question. The model has three purposes:

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<sup>5</sup>Private renters are susceptible to displacement through increasing rent prices. They are a large and growing group in many countries, especially for the young. Even in the UK, which has a relatively high homeownership rate, the private rental share is more than 20% and is growing by about a percentage point each year. Currently in the UK, nearly half of 20-39 year olds rent privately. These shares (and growth rates) are even higher in urban areas. Other countries, like the US (35%) and Germany (40%) already have very high private renter shares.

<sup>6</sup>Working-class urban neighbourhoods are, in fact, part of the common definition of gentrification. Nevertheless, I test if increases in degree share result in displacement in middle-class or suburban neighbourhoods.

to disentangle the concepts of gentrification and displacement, to demonstrate their inherent endogeneity, and to motivate a credible identification strategy. Gentrification in the model is an increase in the share of housing units in an urban neighbourhood occupied by ‘rich’ households. Displacement is defined as ‘poor’ households exiting their current units as a result of such gentrification. The model defines a ‘natural mobility’ as the baseline rate of inflows and outflows even if the shares for each group remain stable in the neighbourhood over time. What the model makes clear is that gentrification (an increase in rich share) must be associated with a decrease in the poor share, given spatial equilibrium and a fixed stock of housing units. However, a key insight is that while this net decrease may occur through increased poor exits (displacement), it might also occur entirely through decreased poor entries below the natural rate (I call this ‘exclusion’). The model illustrates that a no-displacement case occurs if there are sufficiently large moving costs associated with displacement. I am not the first to suggest that gentrification may occur through the natural mobility of a neighbourhood [see e.g. [Vigdor, 2002](#)]. However, my contribution integrates this idea into an appropriate equilibrium sorting framework with heterogeneous preferences within income groups. The advantage of doing so is that it allows for a proper examination of the potential channels of endogeneity between gentrification and poor outflows and for the motivation of the empirical approach.

A second contribution is the IvI approach. I am not the first to use interaction as part of an identification strategy.<sup>7</sup> An example of an existing study that uses a IvI-type approach is [Hilber and Vermeulen \[2016\]](#) who estimate the impact of supply constraints on house prices in the UK by interacting measures of planning restrictiveness with a demand-shifter. Their application is also underpinned by a theoretical model that motivates the interactive relationship. I am, however, the first to propose the IvI as a general approach to ascertain causal effects in cases where a researcher cannot find a valid instrument or discontinuity/kink [see [Angrist et al., 1996](#), [Hahn et al., 2001](#), [Card et al., 2015](#)]. The IvI approach reveals what I call a differential average treatment effect (DATE) that estimates the difference in the average treatment effect (ATE) across values of a moderating variable. This may be thought of as the effect across different intensities or ‘dosages’ of the treatment. As with other quasi-experimental estimates, DATE focusses on a specific source of variation in the treatment in order to estimate an internally valid effect, but does so at the expense of external validity: the average treatment effect (ATE) remains an unknown. As with the

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<sup>7</sup>In some sense the IvI shares similarities with a standard approach in the literature, the difference-in-difference, which uses an interaction of a treatment with a ‘post’ variable to estimate the treatment effect.

local-average treatment effect (LATE), the researcher needs to make a logical justification for how the obtained estimate broadly relate to the ATE. The IvI approach may be most useful where one is interested in knowing *whether* something impacts on another thing (as in my case) rather than *by how much*.

A third important contribution is to provide the first causal evidence of the displacement effects of gentrification. As already discussed, this result has important implications if displacement causes harm to households. Policymakers should think carefully about implications of area-based interventions that result in gentrification if an important justification for the policy is to improve outcomes for existing residents.

The paper structured is as follows. Section II presents the conceptual model of gentrification and displacement. Section III describes the data used in the empirical analysis. Section IV outlines the identification-via-interaction approach. Section V presents the findings.

## II. Conceptual model

This paper tries to answer the question: does gentrification lead to displacement? The usual approach in the literature is to examine exit probabilities for poor households in gentrifying compared with non-gentrifying neighbourhoods. There are three (closely related) concerns with this question and the usual approach. The first is a worry on a conceptual basis that the question itself may be tautological, i.e. that gentrification cannot occur without displacement because they are essentially the same thing. Secondly, even if it is possible to distinguish the concepts, one might worry on a theoretical basis that it simply seems unlikely that gentrification occurs without displacement. Thirdly, even if it accepted that gentrification may lead to displacement in theory, one might be concerned that this will be impossible to test empirically since the variables are highly endogenous. The aim of this section is to address these three concerns and to motivate the empirical approach.

### A. Basic model set-up

There are two city locations indexed  $j = u, s$  (urban, suburban). For simplicity, I assume that the number of housing units is fixed and equal to one in each location.<sup>8</sup> Households choose to consume a unit of housing in one of these two locations to maximise their indirect

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<sup>8</sup>An implication of this assumption is that the urban demands are also urban shares. Another implication is that solving for equilibrium in just the urban location is sufficient for equilibrium overall. Because total demand equals total supply, finding equilibrium in  $J - 1$  locations is enough.

utility that is given by  $v^u = \bar{u}_k - p + \gamma$  in the urban location and  $v^s = \bar{u}_k - \pi$  in the suburban location, where  $\bar{u}_k$  is the utility from consumption of housing units in the suburban location,  $p$  is the rent premium for units in the urban location (over the suburban location),  $\gamma$  is the household-specific utility preference for the amenities specific to the urban location over the suburban location, and  $\pi$  is potential mobility costs for moving to the suburbs.<sup>9</sup> Households will choose the urban location if  $v^u \geq v^s$ , which gives  $\gamma \geq p - \pi$ . Households choose an urban unit if they have a preference level above the urban rent premium minus any potential costs of moving.

Let the distribution of preference parameter  $\gamma$  for each group  $k$  be described by a population density function  $f^k(\gamma)$ . The population of each group is one such that the preference distributions are probability density functions (PDFs). I denote their cumulative density functions (CDFs) as  $F^k(\gamma)$ . I assume that there are two time periods  $t = 0, 1$  and that there may be shifts in the preference distributions from one period to another. I assume that these group shifts preserve the rank of households. Since  $\gamma$  reflects household-specific preferences for urban-specific amenities, a shift in group distribution may reflect both (i) shifts in a group's preferences for urban-specific amenities, or (ii) shifts in urban-specific amenities valued by a group. Thus amenity- and preference-driven explanations for gentrification are captured by changes in the distributions of a single parameter.

To complete the set-up, I assume that there is a probability  $m$  from one period to another that any household will draw a new position (rank) in their group's overall distribution. This last assumption results in natural mobility, where there is probability  $m$  that any household exits the housing unit associated with their previous position and enters a housing unit at a location associated with their new position in the distribution. A within-location moves still occur if the new position is associated with the same location in the new equilibrium.

## *B. Gentrification equilibrium without displacement costs*

To begin, I assume that moving costs are zero. Ignoring moving costs means that households with a preference level above the urban rent premium choose the urban location. This makes the preference level  $\gamma = p_t$  the 'urban boundary'. Urban demands (shares) are given by the population above the urban boundary, which in this case is  $S_t^k = 1 - F_t^k(p_t)$ . Spatial equilibrium is found at the rent level where urban demand shares equal one, which gives

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<sup>9</sup>The suburban utility is the group-specific income minus the suburban rent plus a fixed constant that represents utility derived from the suburban amenity bundle  $\bar{u}_k = y_k - p^s + \gamma^s$ .



$$F_t^p(p_t) + F_t^r(p_t) = 1. \quad {}^{10}$$

Figure 1 plots the poor distribution in  $t = 1$  for a ‘no-gentrification’ and a ‘gentrification’ equilibrium.<sup>11</sup> The no-gentrification case occurs if both rich and poor distributions remain stable over time, i.e.  $F_0^k(\gamma) = F_1^k(\gamma)$ . As a result, both the urban boundary ( $p_0 = p_1$ ) and the demand shares ( $S_0^k(p_0) = S_1^k(p_1)$ ) are stable over time, too. While the shares are stable, there will be a natural mobility that is driven by idiosyncratic changes in preferences, as described above. The amount of natural mobility for poor households in the no-gentrification equilibrium is depicted by area  $M$  in panel (a). Given there is a probability  $m$  of making a natural move, outflows from urban units are  $O^p = mF_0^p(p_0) = M$ , depicted as north-east hatchings.<sup>12</sup> Given natural movers draw randomly from the overall distribution, inflows into urban units are  $I^p = mF_1^p(p_1) = M$ , depicted as north-west hatchings. Thus, even though the shares are fixed over time in the no-gentrification case, there is a churn within the distributions that generates a natural mobility.

The gentrification equilibrium occurs if there is a shift in the rich distribution. Such a shift could correspond to changes in rich preferences or in the amenities that the rich care about.<sup>13</sup> Following the shift, there will be an excess demand for the urban location at the initial rent level  $p_0$  requiring the the urban boundary to shift out to  $p_1$ . Poor natural movers between these two preference levels in  $t = 0$  will leave their urban units (as they would have under no gentrification) but their vacated units will no longer be filled with new poor households. Poor natural movers drawing a preference level between  $p_0$  and  $p_1$  in  $t = 1$  will prefer a suburban unit given increased urban rents. The reduction in inflows resulting from gentrification is given by area  $E = m[F_1^p(p_1) - F_0^p(p_0)]$ , where  $E$  stands for exclusion.<sup>14</sup> Total poor inflows in the gentrification case are equal to natural mobility minus exclusion,  $I^p = M - E$ . Poor natural stayers between  $p_0$  and  $p_1$  will be displaced from their urban units since, at the new rents, the suburban location offers a higher utility. The increase in poor outflows resulting from gentrification is given by area  $D = (1 - m)[F_1^p(p_1) - F_0^p(p_0)]$ , where  $D$  stands for displacement. Total poor outflows in the gentrification case are equal to natural mobility plus displacement,  $O^p = M + D$ .

<sup>10</sup>Waights [2018b] provides some general support for the assumption of spatial equilibrium.

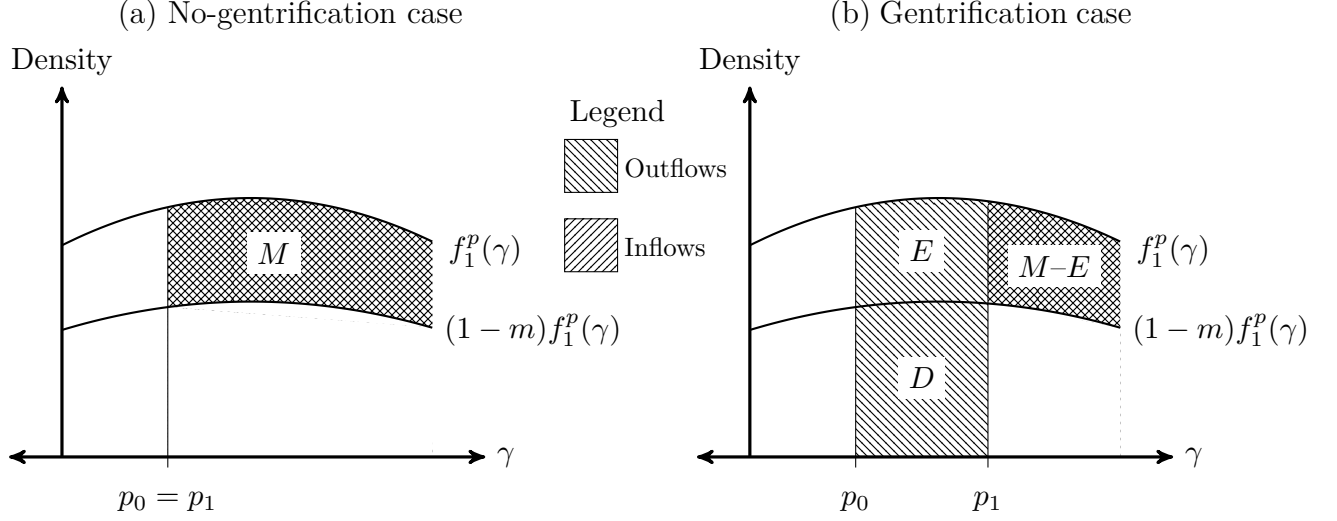
<sup>11</sup>The figure does not plot the whole distribution but only the ‘interesting’ portion.

<sup>12</sup>This correspond to area  $M$  depicted in panel (a) since the distributions are stable over time.

<sup>13</sup>To keep this simple for now, I am implicitly assuming that gentrification is exogenous to poor moves. This means that (i) there is no shift in the poor distribution, and that (ii) the occurrence of gentrification is unrelated to natural mobility  $m$ . I relax this implicit assumption further down.

<sup>14</sup>The existing literature refers to rich households simply moving in after poor households (who were moving anyway) as ‘succession’. I prefer the term exclusion since it puts the emphasis on the reduction of poor inflows rather than an increase of rich inflows which helps to better disentangle different concepts.

Figure 1: No moving costs: no-gentrification and gentrification equilibriums



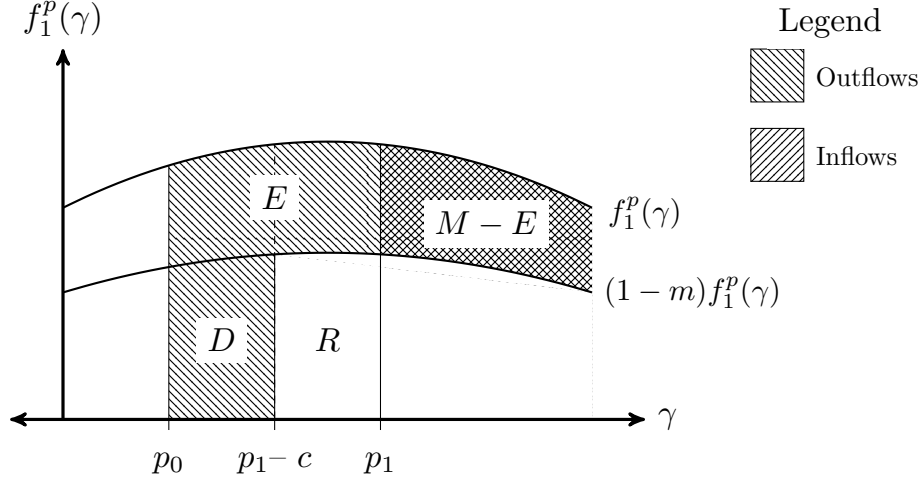
Note that the size of the decrease in the poor share under gentrification may be expressed as poor outflows minus poor inflows which equals exclusion plus displacement  $-\Delta S_1^p = O^p - I^p = E + D$ . Further, note that assuming equilibrium in both periods requires the changes in share to cancel  $\Delta S_1^r = -\Delta S_1^p$ . These two things imply that if the gentrification case is defined as an increase in the rich share, then gentrification requires there to be a positive sum of exclusion and displacement, given equilibrium with a fixed stock. Thus, one thing that the model illustrates so far is that the main question of this paper is not a tautological one because gentrification can be achieved through exclusion as well as through displacement. As the model currently stands, however, gentrification can only occur without displacement in the unlikely case of  $m = 1$  (since otherwise, both  $E$  and  $D$  increase with  $p_1$ ). Therefore, in the case of heterogeneous preferences, simply having natural mobility is not enough for a plausible explanation for the no-displacement case.<sup>15</sup>

### C. Gentrification equilibrium with displacement costs

I assume that moves due to the preference shifts of others (i.e. rent increases) incur a psychic ‘displacement’ cost,  $\pi = c$ , but that moves due to own preference changes (idiosyncratic or group shifts) are costless,  $\pi = 0$ . As discussed, the qualitative literature reports that the psychic costs of displacement may be significant [Slater, 2009]. Figure 2 depicts the

<sup>15</sup>This provides an interesting contrast to the case of homogeneous preferences examined by Vigdor [2002].

Figure 2: Gentrification equilibrium with moving costs



gentrification equilibrium with such displacement costs.<sup>16</sup> Poor natural stayers move to the suburbs only if the cost of staying exceeds the cost of being displaced, i.e. if they have a preference level  $p_0 \leq \gamma < p_1 - c$ . Otherwise, i.e. if  $p_1 - c \geq \gamma > p_1$ , they ‘resist’ displacement, reflected by area  $R$ . It follows that there will be some displaced households so long as displacement costs are smaller than the rent increase,  $c < p_1 - p_0$ . However, a no-displacement case occurs if displacement costs exceed the rent increase,  $c \geq p_1 - p_0$ . In such a no-displacement case, gentrification occurs entirely through exclusion, illustrated by area  $E$ . Exclusion is unaffected by displacement costs since poor natural movers are changing housing unit as a result of their own idiosyncratic changes in preferences. Therefore, they locate in the urban location only if their preference level is above the new boundary at the increased rent level  $p_1$  (just as in the costless case).

Spatial equilibrium in the second period is now given by:

$$F_1^r(p_1) + mF_1^p(p_1) + (1 - m)F_1^p(p_1 - c) = 1, \text{ if } c < p_1 - p_0 \quad (1)$$

$$F_1^r(p_1) + mF_1^p(p_1) + (1 - m)F_1^p(p_0) = 1, \text{ if } c \geq p_1 - p_0 \quad (2)$$

where in the no displacement case (the second one), the urban boundary for natural stayers simply stays at  $p_0$  in the second period.<sup>17</sup>

<sup>16</sup>I plot only the gentrification equilibrium since the no-gentrification case is broadly the same as without moving costs.

<sup>17</sup>It cannot be to the left of  $p_0$  since households need to begin in the urban location to resist displacement.

### D. *An identification problem*

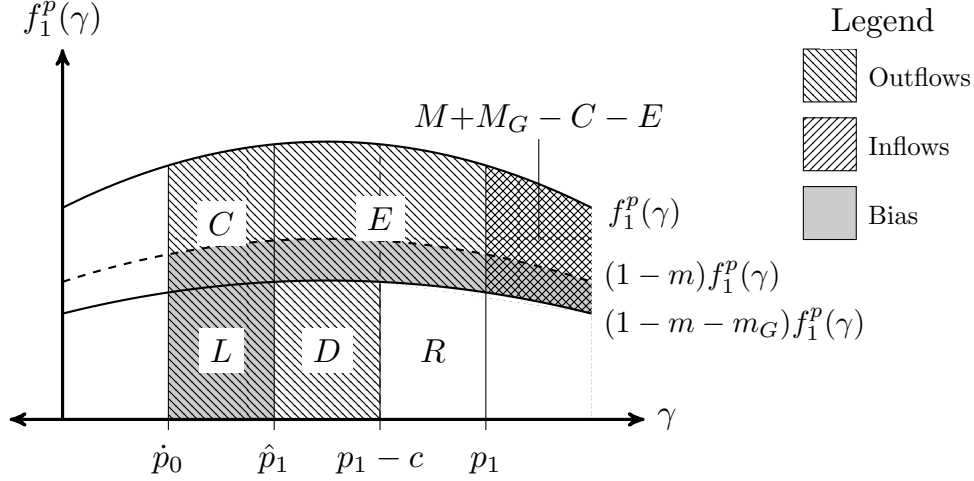
Note that up until this point in the model, the usual empirical approach is a consistent one. A comparison of poor outflows in the gentrification case to the no-gentrification case does reveal displacement, i.e. area  $D$ . However, this is only the case because I have so far assumed gentrification to be exogenous to poor mobility. This assumption may not be completely realistic for two reasons: selection, and reverse causality. Selection occurs because gentrifying neighbourhoods are not a random selection of neighbourhoods. According to [Buck \[2001\]](#), they are typically more central, with better transport access and older dwelling stock than other neighbourhoods. Therefore, given the presence of neighbourhood sorting, it is likely that certain type of household with a certain mobility rate would be living in these neighbourhoods even before any gentrification occurred. The benefits of explicitly modelling such pre-existing sorting patterns would not justify the required additional model complexity. Therefore, I choose to simply let  $m_G$  represent the difference in natural mobility in gentrifying neighbourhoods resulting from such hypothesised sorting. Figure 3 illustrates a model equilibrium where natural mobility has increased in the gentrification case resulting in the smaller distribution of natural stayers, drawn by the solid line  $(1 - m - m_G)f^p(\gamma)$ . For comparison, the counterfactual distribution of natural stayers in the no-gentrification case is drawn by the dashed line  $(1 - m)f^p(\gamma)$ .

Reverse causality happens if poor outflows lead to gentrification rather than the other way around. So far I have assumed that change in rich preferences (or amenities that the rich like) are the sole cause of changes in neighbourhood shares. However, in reality the poor may like (or even dislike) the same urban amenities that the rich like. Further, changes in preferences for certain amenities may be correlated (positively or negatively) between the groups. Finally, endogenous amenities may play a role such that an initial change in preferences and neighbourhood share in one group, leads to further changes because of each group's preferences for neighbourhood composition [e.g. [Brueckner et al., 1999](#), [Guerrieri et al., 2013](#)]. Figure 3 illustrates an equilibrium where gentrification is the result of an increase in rich preferences and a decrease in poor preferences for the urban location. The effect of the poor preference shift is represented by  $\hat{p}_1$  which is an intermediate equilibrium that holds the rich distribution constant.<sup>18</sup> Poor natural stayer households between the initial

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<sup>18</sup>This intermediate equilibrium might be imaginary or real. It is imaginary if rich and poor shifts occur simultaneously. In this case, we hold the rich distribution constant in order to imagine the counterfactual where the rich distribution did not shift. It could also be a real equilibrium if the poor equilibrium shifted first, and then the rich equilibrium shifter (perhaps as a result of an initial change in poor share under the case of endogenous amenities). In both cases the intermediate equilibrium helps ‘causal accounting’—to see

Figure 3: Gentrification equilibrium poor decrease and rich increase in urban preference



urban boundary  $\dot{p}_0$  and the intermediate equilibrium  $\hat{p}_1$  will exit their urban units and move to the suburbs as a result of their own decrease in preferences for the urban location.<sup>19</sup> These ‘leavers’ are illustrated by area  $L$  on the figure. Poor natural movers between these preference levels, i.e. area  $C$ , will ‘change’ the destination of a move (that they were already making) from an urban to the suburban unit as a result of a change in their own preferences.

Clearly, both selection and reverse causality are associated with increased poor exits that cannot be attributed as an ‘effect’ of gentrification. In order to examine the size of this bias, I make the following distinction. I define a ‘treatment’ group  $T$  as poor urban households in the case where the urban neighbourhood gentrifies. The treatment itself is the upwards shift in the rich distribution. Poor outflows in the treatment group when it is treated are  $O_1^T = D + M + M_G + L$ . Poor outflows in the treatment group if it were not treated would be  $O_0^T = M + M_G + L$ . The difference is the treatment effect and equals displacement  $O_1^T - O_0^T = D$ . However, the treatment group is not observed in the non-treated state. The usual empirical approach is to compare outflows in the treatment group to a non-treated group. I define a control group  $C$  that is the poor urban households in the non-gentrification case. This group is untreated and has outflows  $O_0^C = M$ . The difference between the treated and control groups and the estimated displacement effect is  $O_1^T - O_0^C = D + M_G + L$ . Hence the estimated displacement effect is biased by the areas  $M_G$  and  $L$  highlighted in grey on

analytically which poor moves result *from* gentrification and which result *in* gentrification.

<sup>19</sup>The initial urban boundary is denoted  $\dot{p}_0$ , where the dot signifies shift in the distribution. This preference level is the rank (or position) in the overall distribution of the initial urban boundary, but does not equal the preference level  $p_0$  since the distribution has shifted (in this case downwards).

the figure.

To some extent the area  $M_G = m_G F^p(p_0)$ , may be controlled for in an estimation with household characteristics. The difference in natural mobility rate in gentrifying neighbourhoods might be written as  $m_G = m_x x_G + m_u u_G$  where  $x_G$  is the difference in observable household characteristics and  $u_G$  the difference in unobservable characteristics in gentrifying neighbourhoods as a result of sorting, and  $m_x$  and  $m_u$  are the partial derivatives between characteristics and natural mobility. The overall mobility difference may then be written as  $M_G = m_x x_G F^p(p_0) + m_u u_G F^p(p_0)$ , where inclusion of household characteristics in estimation controls for the first term but not the second term.

### *E. Motivating identification-via-interaction*

My identification strategy in the empirical section is based on the interaction between the gentrification treatment and a random negative budget shift of size  $b$  that occurs in the second period. Identification relies on the interaction being ‘valid’ in that (a) there is an interactive effect with the budget shifter for the relationship of interest, i.e. displacement, and that (b) there are no interactive effects with the budget shifter for the nuisance relationships, i.e. natural mobility and voluntary leaves. If these validity conditions hold then a positive interaction effect can only be the result of displacement. In the empirical section I will borrow terms from the instrumental variables literature in calling these two validity conditions the ‘relevance’ condition and the ‘exclusion restriction’. For now, the model section aims simply to motivate the approach and to formulate the identifying assumptions.

Firstly, I use the model to illustrate how the displacement effect increases with a negative budget shifter. I assume that  $c$  increases in the total household budget, i.e.  $c = c(y^p - b)$  where  $y^p$  is poor household income and  $b$  is the negative budget shift. The justification for this assumption is that households on a tighter budget will be less able to forgo consumption goods to avoid the psychic cost of displacement because their consumption goods comprise a larger share of essentials such as food, clothing and transport. The implication is that the negative budget shift increases vulnerability to displacement since  $c_b < 0$  which leads to the following proposition:

**Proposition 1:** Displacement effects are interactive with the budget shifter, i.e.  $\frac{dD}{db} > 0$ , and if there are no displacement effects then  $\frac{dD}{db} = 0$ .

To demonstrate Proposition 1, consider that the amount of displacement in Figure (3) is

given by  $D = (1 - m - m_G) [F^p(p_1 - c) - F^p(\hat{p}_1)]$  in the displacement case, i.e. if  $p_1 - c \geq \hat{p}_1$ , and  $D = (1 - m - m_G) [F^p(\hat{p}_1) - F^p(\hat{p}_1)] = 0$  in the no displacement case, i.e.  $p_1 - c < \hat{p}_1$ .<sup>20</sup> The partial derivative of displacement with respect to the budget shifter is therefore:

$$\frac{dD}{db} = \begin{cases} (1 - m - m_G) [F_\gamma^p(p_1 - c)(\frac{dp_1}{db} - c_b) - F_\gamma^p(\hat{p}_1)(\frac{\hat{p}_1}{db})] > 0, & \text{if } p_1 - c \geq \hat{p}_1 \\ 0, & \text{if } p_1 - c < \hat{p}_1. \end{cases} \quad (3)$$

Thus if there are any displacement effects then they increase with the budget shifter.<sup>21</sup>

The partial derivative has ignored any direct effect of the budget shifter on natural mobility, for simplicity. Allowing for  $m_b \neq 0$  adds two terms to partial derivative in the displacement case of equation (3) because the scope for being displaced is reduced if natural mobility is higher.<sup>22</sup> However, this is an empirical concern only for the ‘relevance’ of the interaction rather than for the ‘exclusion restriction’. The worst manifestation of this problem would be if the budget shift has a large enough positive impact on natural mobility that it ‘wipes out’ any increase in displacement effect.<sup>23</sup> A positive and significant interaction term would be sufficient evidence to alleviate this concern (conditional on the exclusion restriction).

Secondly I use the model to illustrate a second proposition that:

**Proposition 2:** The endogenous channels are not interactive with the budget shifter

$$\frac{dM_G}{db} = \frac{dL}{db} = 0$$

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<sup>20</sup>Note that the urban boundary for natural stayers facing displacement cannot fall to the left of  $\hat{p}_1$  since those leaving voluntarily cannot be displaced.

<sup>21</sup>I show that the first case of equation (3) is positive by showing that the first term inside the square bracket is positive and that the second term is zero. Taking the differential of the equilibrium condition, equation (1), with respect to the budget shifter gives

$$F_\gamma^r(p_1) \frac{dp_1}{db} + m F_\gamma^p(p_1) \frac{dp_1}{db} + (1 - m) F_\gamma^p(p_1 - c) (\frac{dp_1}{db} - c_b) = 0$$

which rearranged gives  $\frac{dp_1}{db} = \frac{(1-m)F_\gamma^p(p_1-c)}{F_\gamma^r(p_1)+mF_\gamma^p(p_1)+(1-m)F_\gamma^p(p_1-c)} c_b < 0$  since  $c_b < 0$ . Rearranging again gives  $\frac{dp_1}{db} - c_b = -\frac{F_\gamma^r(p_1)+mF_\gamma^p(p_1)}{(1-m)F_\gamma^p(p_1-c)} \frac{dp_1}{db} > 0$  since  $\frac{dp_1}{db} < 0$ . Therefore the first term in equation (3) is positive. Next, note that the intermediate equilibrium  $\hat{p}_1$  is found where  $F_0^r(\hat{p}_1) + F_1^p(\hat{p}_1) = 1$  which is unrelated to the budget shifter  $b$  since it does not depend on  $c(y^p - b)$  or  $p_1$ . Thus the second term is zero and equation (3) is positive overall.

<sup>22</sup>Displacement changes if the budget shifter impacts on natural mobility because it result in a shifting of the top edge of area  $D$ .

<sup>23</sup>A negative impact on natural mobility may inflate the size of the interaction. However, as will be discussed in the empirical section, the focus is on the sign and significance rather than the magnitude of the interaction effect.

To demonstrate Proposition 2 consider that the selection bias is area  $M_G = m_G F^p(\dot{p}_0)$  and its partial derivative is:

$$\frac{dM_G}{db} = m_G F_\gamma^p(\dot{p}_0) \frac{d\dot{p}_0}{db} + m_{Gb} F^p(\dot{p}_0) = 0 \quad (4)$$

This first term is zero because the initial urban boundary will be independent of the random budget shock in the second period such that  $\frac{d\dot{p}_0}{db} = 0$ . The second term is zero only if the difference in natural mobility between gentrifying and non-gentrifying neighbourhoods is not interactive with the budget shifter, i.e.  $m_{Gb} = 0$ . This is a key identifying assumption of the empirical approach. A basis for this assumption is to write the difference in natural mobility as a result of sorting on household characteristics, i.e.  $m_G = m_x x_G + m_u u_G$  as above. The interaction with the budget shifter is  $m_{Gb} = m_x x_{Gb} + m_u u_{Gb} + m_{xb} x_G + m_{ub} u_G$ . Thus the assumption holds if the budget shifter impacts on neither the characteristics of households that live in gentrifying neighbourhoods nor the relationships better characteristics and mobility.

The first requirement, that  $x_{Gb} = u_{Gb} = 0$ , may be considered reasonable if sorting into neighbourhoods is something that occurs over the long-run mostly in response to neighbourhood characteristics, rather than something that reacts quickly to sudden and random year-to-year shifts in the budget. An easy empirical check would be to exclude all households who arrived in the neighbourhood in the same year as the budget shift. The second requirement, that  $m_{xb} = m_{ub} = 0$ , may be considered reasonable if household characteristics have a constant relationship with mobility rates. Interacting the budget shifter with household characteristics would provide an indication of the validity of this assumption. If the control interactions are largely insignificant and if the treatment interaction doesn't change much then this wouldn't appear to be a major worry.

The reverse causality bias is area  $L = (1 - m - m_G) [F^p(\hat{p}_1) - F^p(\dot{p}_0)]$  and its partial derivative with the budget shifter is

$$\frac{dL}{db} = (1 - m - m_G) \left[ F_\gamma^p(\hat{p}_1) \frac{d\hat{p}_1}{db} - F_\gamma^p(\dot{p}_0) \frac{d\dot{p}_0}{db} \right] = 0. \quad (5)$$

This partial derivative shows that the reverse causality effect, too, is unaffected by the budget shifter. However, as with the displacement effect, I have ignored any effect of the budget shifter on natural mobility. Allowing for  $m_b \neq 0$  adds an additional term to equation ( ) because the scope for leaving voluntarily is reduced (increased) if natural mobility is higher



(lower). If the natural leave component is a significant part of overall gentrification, then a significant and negative effect of the budget shifter on natural mobility is a source of potential bias for the interaction term.

One might think that gentrification is typically driven by the rich and not the poor, making reverse causality a lesser concern than selection bias, generally. However, it may be that in certain neighbourhoods, observed gentrification is at least partly the result of the poor group voluntary leaves. If this were the case then these neighbourhoods would witness smaller rent increases compared with neighbourhoods where preference increases by the rich group is the sole cause. Therefore, if a convenient empirical test is to see if the interaction effect is larger in neighbourhoods with slower increase in price. Finally, it would further alleviate this concern would if the budget shifter does not appear to have a significant effect on natural mobility.

### III. Data

I examine the displacement effects of gentrification using households from the British Household Panel Survey (BHPS). The BHPS is a representative longitudinal survey of more than 5,000 households in each year over the period 1991-2008. The dataset contains detailed household information as well as geographical location. The geographical location allows for merging with a measure of neighbourhood gentrification defined in the next section. Interviews are conducted with the household heads and all other household members of age 16 years or over. Households are reinterviewed in subsequent waves regardless of whether they change location.<sup>24</sup> I use head of households observed in a year as the unit of observation. The dependent variable for the analysis is whether the head of household changes address. This variable is equal to one if the head is observed again in the next wave and answers ‘no’ when asked if living at the same address last year. The variable is coded as missing if the head is not observed again in the next wave.<sup>25</sup> My main analysis focuses on households who rent privately, since this is the tenure type that is susceptible to displacement through increased housing costs. I define private renting as those that report their landlords to be private individuals or companies, as opposed to local authorities, housing associations, relatives, or employers. Finally, in order to further ensure vulnerability to displacement, I drop

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<sup>24</sup>If there is a household split, only the head of household is followed across waves with all the members of their new household being interviewed.

<sup>25</sup>I provide support for results for self-reported moves in a sensitivity test that uses an alternative outcome variable based on change in location as recorded by the interviewer.

households in receipt of housing benefit. The resulting private renter sample has 5,362 head of household-year observations. Table 1 reports summary statistics of this dataset.

My empirical analysis investigates the extent to which displacement effects are conditional on household income. For this purpose I generate an income index. I divide household income by the number of household members reported in the BHPS to get household income per capita. I then divide the result by the average income for the city (travel-to-work-area) and year based on fixed effects estimated in a separate regression. In my main baseline specification I restrict the sample to households with an income of less than the average for their city and year. In alternative specifications, I investigate effects across the whole income range.

In order to characterise neighbourhood gentrification I make use of data from the UK Census for 1991, 2001 and 2011. In particular, I use the intercensal change in the share of residents in a neighbourhood that hold a university degree. Both income and educational attainment have been used in the literature to measure gentrification.<sup>26</sup> However, educational attainment is a more stable personal characteristic than income and therefore serves as a more reliable measure of an inflow of a different demographic group rather than simply changes in the characteristics of existing residents. The degree share variable is obtained at the Middle Layer Super Output Area (MSOA) level for England and Wales and the (approximately equivalent) Intermediate Zone (IZ) level for Scotland. I use the MSOA/IZ areas as my neighbourhood definition. Together there are 8,429 such neighbourhoods across Great Britain. The average number of households that live in an MSOA is around 3,000 and for IZs it is around 4,000.<sup>27</sup> These areas are comparable in size to recent studies that make use of non-public census data for the US [McKinnish et al., 2010, Freeman, 2005].

I compute a gentrification index as:

$$G_{nc} = \Delta D_{nc} - \frac{\sum_n \Delta D_{nc}}{N} \quad (6)$$

where  $\Delta D_{nc}$  is the change in degree share for neighbourhood  $n$  over intercensal period  $c$ , and  $N = 8,429$  is the total number of neighbourhoods. Figure 4 maps the resulting gentrification index for the 1991-2001 period for neighbourhoods in London. Figure 5 does the same for the 2001-2011 period. London gentrification in the 1990s is solidly concentrated in central

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<sup>26</sup>For example Ahlfeldt et al. [2017] and Waights [2018a] both use degree share change from the UK census as a measure of gentrification.

<sup>27</sup>Where boundaries changed over time, the relevant geographies were calculated based on aggregation of smaller geographies. Any remaining overlaps were dealt with by apportioning according to the number of postcodes in each area part.

neighbourhoods. In the 2000s, the gentrification occurs mostly in London’s outer parts and its east central neighbourhoods. The reason for this pattern is that most neighbourhoods that gentrified in the 1990s are fully gentrified by the 2000s, such that gentrification can only spread further out.

The demeaning of the degree share is intended to remove any influence of general increases in educational attainment to further ensure that the index captures inflows of a different demographic group rather than changes in characteristics of existing residents. Increases in the neighbourhood degree share above the national average are more likely to be the result of net inflows of ‘rich’ or ‘middle-class’ residents, i.e. gentrification. Conversely, increases below the national average likely to be associated with net outflows of ‘rich’ or ‘middle-class’ residents, which is the reverse of gentrification.

I use a continuous rather than the binary measure of gentrification. While the conceptual model deals with gentrification in a binary sense, one of the key results is that the existence of displacement depends on the size of the rent increase relative to the size of displacement costs. Therefore, it follows that the precise pace of gentrification is important for displacement effects, motivating the use of a continuous measure in empirics.<sup>28</sup> In my main specifications, I drop residents living in neighbourhoods where the gentrification index is negative, which equates to approximately half the overall sample. This justified since reverse gentrification is not a focus of the question at hand nor analysed in the theoretical model. However, in non-linear specifications, I estimate displacement effects across the full range of gentrification index values.

I use data on heating degree days as the budget-shifter for my main analysis. Degree days are used in the energy industry as an indicator of the amount of energy needed to heat buildings based on outside temperature. I obtain degree days for the UK for each year in my observation window from the Department of Energy and Climate Change (DECC). The DECC creates degree days as the sum across days in a year of the number of degree Celsius that the daily mean temperature falls below 15.5.<sup>29</sup> The left plot of Figure 6 shows the raw degree days data divided by 365. Dividing by the number of days in a year gives an indication of the deviation below 15.5 degrees Celsius on an average day in a given year. Note that a one degree increase in the measure might correspond to 2 degrees colder over 6 months where heating is necessary, or 12 degrees colder in just one of those months.

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<sup>28</sup>I show that results are comparable when using a binary outcome in a sensitivity test.

<sup>29</sup>An outside temperature of 15.5 degrees Celsius corresponds to an inside temperature of 18 degrees Celsius which is considered to be a threshold temperature below which space requires heating. Days that have a mean temperature above 15.5 degree Celsius have a degree days score of zero.

The right plot shows the rescaled version of the degree days variable that I use in my empirical analysis. The variable is rescaled to represent the impact of degree days on the budget through fuel bills. The rescaling procedure begins with detrending and demeaning the series.<sup>30</sup> I then scale (multiply) the series by its coefficient from a regression on reported fuel bills for households in my sample. Table 2 reports the regression of fuel bills (expressed in hundreds of pounds) on degree days and some alternative and placebo budget-shifters. The degree days estimates in column (1) show that annual fuel bills are £209 (approximately \$335) higher per unit increase. This extra amount spent on heating corresponds to about two additional weeks of rent or 1.2 percent of annual gross household income for the average household in the sample. This analysis suggests that even low-income private renter households react to cold weather by spending significant extra amounts on heating. Households must spend more on heating if they are to avoid inside temperatures falling together with outside temperatures, especially since risk of respiratory disease begins below 16 degrees and of hypothermia below 9 degrees. Therefore, the degree days measure of cold weather provides significant variation in the household budget.

Degree days is a measure that is specifically constructed based on weather variation to predict variation in heating costs. However, one might worry that empirical results are affected by the particular the variable is constructed. In order to address such concerns I make use of two alternative budget-shifters: average winter temperature and average annual temperature. These simpler reflections of temperature variation are also expected to impact on fuel bills. Columns (2) and (3) in Table 2 confirm this is the case. Note that the signs are the opposite to that of degree days because an increase in degree days reflects colder (smaller) temperatures. I also estimate alternative specification with two placebo budget-shifters: rainfall and sunshine. These address a worry that there is something particular about the time-series data structure or weather factors that might drive an effect other than through the hypothesised budget channel. Columns (4) and (5) illustrate that these placebo budget-shifter indeed have no effect on fuel bills. As with degree days I re-scale these alternative and placebo budget-shifter based on their coefficients in the fuel bills regression. The resulting series reflect year-to-year change to fuel bills predicted by cold weather.<sup>31</sup>

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<sup>30</sup>Detrending and demeaning is achieved by obtaining the residuals from a regression of the series on a time trend and a constant. The downwards trend has a slope of 0.04 degrees Celsius which is a little higher than estimates of the current rate of global warming of about 0.02-0.03 degrees Celsius per year. Note, however, that the trend becomes a little less steep when widening the observation window to consider more recent observations.

<sup>31</sup>It should be noted that this scaling procedure is similar to a two-stage least squares regression that uses fuel bills as the budget shifter, instrumenting them with degree days. However, not enough households in

Finally, Figure 7 shows the correlation between the preferred budget-shifter degree days and the alternative and placebo budget-shifters. As one would expect the alternative shifters are strongly (negatively) correlated. However, one of the placebo budget shifters, rainfall, is also correlated with degree days ( $p < 0.1$ ). As such rainfall will likely work as a placebo only when controlling for degree days. Sunshine on the other hand is uncorrelated and provides a cleaner placebo test.

## IV. Identification-via-interaction

A common empirical problem is illustrated by the following model

$$Y_i = \beta T_i + \delta X_i + e_i \quad (7)$$

where  $Y_i$  is an outcome variable of interest for unit  $i$ ,  $T_i$  is a treatment variable,  $X_i$  are control variables, an  $e_{in}$  is a residual error term. This specification with household exits as the outcome and gentrification as the treatment broadly captures the usual approach taken in the literature to answer the displacement question. The parameter  $\beta$  measures the effect of the treatment under the identifying assumption that  $Cov(T_i, e_i) = 0$ . Let the error term be comprised of unobservable determinants of the outcome variable and a white noise error term:  $e_i = \alpha U_i + \epsilon_i$ . Let the treatment variable be endogenous to such unobservables in a model such as

$$T_i = \theta U_i + \zeta_i \quad (8)$$

where  $\theta$  is the relationship between the unobservables and the treatment, and  $\zeta_{in}$  is another white noise error term. This equation is the empirical equivalent to the endogenous relationships illustrated in my conceptual model, with  $U_i$  representing the household factors that determine natural mobility and the decision to leave a neighbourhood voluntarily. If we rearrange equation (8) for  $U_i$  and plug it into the equation (7) we reveal the key estimate to be:  $\hat{\beta} = \beta + \frac{\alpha}{\theta}$  which is inconsistent. In my case, the bias  $\frac{\alpha}{\theta}$  represents the selection and reverse causality highlighted in the conceptual model. In this section, I propose IvI as a general solution to this common empirical problem, and apply it in developing empirical specification for my specific case.

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the sample report fuel bills to be able to consistently estimate 2SLS. Therefore, I prefer to stick to reduced form estimation using rescaled degree days.

### A. The IvI approach

The IvI approach involves estimating the following model

$$Y_i = \beta_D(T_i \cdot Z_i) + \beta_A T_i + \beta_Z Z_i + \pi X_i + r_i \quad (9)$$

where  $Z_i$  is a moderating variable that indicates vulnerability to the treatment. The criteria for the validity of the interaction are that (i) the moderating variable impacts on the size of the treatment effect, and that (ii) the moderating variable does not impact on any endogenous relationships. The first criterion is the ‘relevance’ condition and the second is the ‘exclusion restriction’, to borrow terms from the instrumental variables (IV) literature.

If the residual is written as  $r_i = \alpha_D(U_i \cdot Z_i) + \alpha_A U_{intc} + \varepsilon_i$  then the second criterion means that  $\alpha_D = 0$ . Substituting the endogenous relationship—equation (8)—into this residual reveals the estimating equation to be

$$Y_i = \left[ \beta_D + \frac{\alpha_D}{\theta} \right] (T_i \cdot Z_i) + \left[ \beta_A + \frac{\alpha_A}{\theta} \right] T_i + \beta_Z Z_i + \pi X_i + \left[ \varepsilon_i - \frac{\alpha_D}{\theta} (\zeta_i \cdot Z_i) - \frac{\alpha_A}{\theta} \zeta_i \right]. \quad (10)$$

Clearly,  $\hat{\beta}_D = \beta_D + \frac{\alpha_D}{\theta}$  is a consistent estimate of  $\beta_D$  if  $\alpha_D = 0$  holds. The second criterion is equivalent to an exclusion restriction of  $Cov[T_i \cdot Z_i, r_i] = 0$ . As with the IV approach, relevance and the exclusion restriction should be carefully justified. One may choose to do this with a conceptual model and/or a logical argument.

If the interaction is valid  $\beta_D$  is a consistent estimate of what I term the differential average treatment effect or DATE. The DATE provides an estimate of the difference in the average treatment effect (ATE) across units with different vulnerabilities to the treatment. The DATE is useful to the extent that we are interested in effect heterogeneity across the moderating variable, or to the extent that it can tell us something about the ATE. Thus there is a further parallel with the IV (or regression discontinuity design) literature in that the researcher should think carefully about how the obtain estimate relates to the ATE. In both cases, one uses a specific form of variation in order to achieve internal validity, but does so to some extent by sacrificing external validity.

### B. Baseline specification

My empirical specification are based on the identification-via-interaction approach outlined above. My baseline specification interacts the gentrification ‘treatment’ with the budget

shifter moderating variable in the following estimation:<sup>32</sup>

$$M_{intc} = \lambda_D(G_{nc} \cdot B_t) + \lambda_A G_{nc} + \lambda_B B_t + \sigma H_{it} + d_c + \phi_{intc} \quad (11)$$

where  $M_{intc}$  is a move indicator for household  $i$ , living in neighbourhood  $n$ , observed in year  $t$  and intercensal period  $c$ ,  $G_{nc}$  is the gentrification indicator measured as the demeaned increase in share of resident holding a university degree in neighbourhood  $n$  over intercensal period  $c$ ,  $B_t$  is the degree days budget-shifter outlined in the data section,  $H_{it}$  are household controls,  $d_c$  is a decade dummy for the second intercensal period (I show robustness to using year and fixed effects), and  $\phi_{intc}$  is the residual error term.

As outlined above, the validity of the interaction should be justified. In my case the motivation for the interaction is based on the conceptual model. The model suggests that the interaction is relevant since, in years when budgets are already stretched, households will be less able to resist financial pressure associated with increasing housing costs. Therefore, if displacement effects are present then  $\lambda_D > 0$  but if there are no displacement effects then  $\lambda_D = 0$ . The model also motivates the exclusion restriction  $Cov[G_{nc} \cdot B_t, \phi_{intc}] = 0$  by illustrating that the endogenous channels are unaffected by the budget shifter. The key intuition is that a time-vary random budget shift does not impact on the household characteristics that determine natural mobility or the decision to leave voluntarily. As discussed, however, further threats to the exclusion restriction should be carefully considered.

### *C. Exclusion restriction*

The exclusion restriction is violated if the budget shifter impacts on moves differentially with gentrification for any reason other than displacement. The model highlights some potential concerns in this regard that motivate alternative specifications. One such concern is if the budget shifter impacts on the characteristics of households that live in gentrifying neighbourhoods or on the relationships between such characteristics and natural mobility. This might be considered plausible if the budget shifter cause certain types of households to move and if these households are simply concentrated in gentrifying neighbourhoods. Two alternative specifications investigate these threats: (i) additionally controlling for the interaction between the household characteristics and the budget shifter, and (ii) excluding households arriving in the same year as the budget shift. A further threat highlighted by the model is that the budget shifter could increase the reverse causality channel if it has a direct

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<sup>32</sup>I use OLS but the results are robust to using probit and logit.

and negative effect on natural mobility. However, if higher exit rates under gentrification is driven an increase in voluntary leaves, then this effect is expected to be associated with slower rental increases compared to neighbourhood where gentrification is driven by increasing rich preferences. Therefore, I address this threat with a sub-sample analysis for neighbourhoods with slower and faster rent increases.

Whilst the model is useful for thinking through potential violations of the exclusion restriction, there will away be a need for careful consideration of further threats to identification. Since the budget shifter is a measure of cold whether, it may have effects on moves not just because of budget effects but also because of dissatisfaction with housing (e.g. if insulation or heating is poor). A concern for the exclusion restriction would be if dissatisfaction with housing resulting from cold weather is greater in gentrifying neighbourhoods because the dwelling stock there is typically older and potentially more draughty and poorly insulated. I address this concern with a specification that includes the budget-shifter interacted with the share of the neighbourhood dwelling stock that was build in the pre-1900 or 1939 periods.

#### *D. Is DATE useful?*

Displacement expressed as an average treatment effect (ATE) in my model is  $\beta_A + \beta_R \times \bar{B}_t$  where  $\bar{B}_t$  is the average value for the budget shifter. While I obtain a consistent estimate of  $\beta_R$ , which I call the differential average treatment effect (DATE), I am unable to obtain an unbiased estimate of the constant term for the ATE,  $\beta_A$ . I argue that DATE is useful in this context because, following the conceptual model, DATE reflects the sign and relative magnitude of the ATE. The DATE is zero when the ATE is zero, and the DATE gets bigger when the ATE gets bigger. Thus my empirical approach is useful in assessing *whether* there is a causal effect at play, but not necessarily the *absolute magnitude* of such an effect. An underlying assumption, however, is that the primary channel for displacement effects is through increased housing costs related to gentrification. According to the model, such housing costs displacement will be differential with the budget-shifter. However, there is a threat to the usefulness of obtained estimates if there are substantial causal effects of gentrification on poor exits that are *not* differential with the budget shifts.

Other than through housing costs, gentrification may impact on mobility through changes to neighbourhood amenities. On the one hand gentrification may lead to amenity changes that increase the likelihood of moving i.e. if existing residents feel alienated [Lees et al., 2013]. This could express itself is as a sort of tipping, the reverse of that recorded by Card et al.



[2008]. On the other hand gentrification may lead to improvements to a neighbourhood that make existing residents *less likely* to move [Vigdor, 2002, 2010]. The second of these cases is the greater worry since it could result in a situation where the DATE finds a differential displacement effect with the budget-shifter but where the ATE is not positive. However, such a situation is unlikely for my sample where the average income of low-income private renters is £13,310 in 2011 prices. According to evidence provided by Vigdor [2010], the willingness-to-pay for neighbourhood improvements associated with gentrification are much smaller than actual rent increases for households with such low incomes.<sup>33</sup> Another potential worry is if gentrification impacts on poor exits through employment opportunities. This, too, does not appear to be a major worry according to existing evidence which finds jobs effect of gentrification to be either small or negative [Lester and Hartley, 2014, Meltzer and Ghorbani, 2017].

### E. Additional specifications

I investigate the how displacement effects vary with household income. I estimate the following semi-parametric model:

$$M_{intc} = \sum_h \lambda_{Dh}(G_{nc} \cdot B_t \cdot I_{h,it}) + \sum_h \lambda_{Ah}(G_{nc} \cdot I_{h,it}) + \sum_h \lambda_{Bh}(B_t \cdot I_{h,it}) + \sum_h \lambda_h I_{h,it} + \sigma H_{it} + d_c + \phi_{intc} \quad (12)$$

where  $I_{h,it}$  are 5 bins of 0.4 width for household income per capita relative to the average for the city and year (as described in data section). I also estimate a model where the differential

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<sup>33</sup>Vigdor [2010] estimates that neighbourhood quality improvements associated with urban revitalisation in US cities over 1985–1993 resulted in an increase in annual rents of around \$600 in 1993 prices. Using a revealed preference approach he estimates that willingness-to-pay for such improvements is larger than actual increases for most households, but that it decreases with income. The willingness-to-pay was only \$300 for highschool dropout households earning only \$30,000 per year (the most vulnerable of the household types that such statistics were provided for) suggesting that for these households, the improvements were not worth the cost. In my sample of low-income renters, the average income is £13,310 in 2011 prices, which corresponds to approximately the same figure in 1993 dollars. (Deflating to 1993 using a factor of 0.675 then converting to dollars using the 1993-average GBP-USD exchange rate of 1.502 gives \$13,476 in 1993 dollars.) Thus, the willingness-to-pay for such a low-income group is likely to be a small fraction of the actual rent increases.

displacement effect is conditional on a household income binomial:

$$\begin{aligned}
M_{intc} = & \lambda_D(G_{nc} \cdot B_t) + \lambda_{D1}(G_{nc} \cdot B_t \cdot I_{it}) + \lambda_{D2}(G_{nc} \cdot B_t \cdot I_{it}^2) \\
& + \lambda_A G_{nc} + \lambda_{A1}(G_{nc} \cdot I_{it}) + \lambda_{A2}(G_{nc} \cdot I_{it}^2) \\
& + \lambda_B B_t + \lambda_{B1}(B_t \cdot I_{it}) + \lambda_{B2}(B_t \cdot I_{it}^2) + \sigma H_{it} + d_c + \phi_{intc}
\end{aligned} \tag{13}$$

where  $I_{it}$  is relative household income.

As discussed in the data section, the baseline specification makes use of a sample that excludes households in neighbourhoods where the gentrification index is negative, i.e. those where the increase in degree share is below-average. In alternative specifications, I investigate the non-linear effects of gentrification using the full sample of private renter households (i.e. also those in neighbourhoods with negative values of the gentrification index). I estimate a semi-parametric model:

$$M_{intc} = \sum_{b=1}^9 \lambda_{Db}(G_{b,nc} \cdot B_t) + \sum_{b=1}^9 \lambda_{Ab} G_{b,nc} + \lambda_B B_t + \sigma H_{it} + d_c + \phi_{intc} \tag{14}$$

where  $G_b$  are 9 bins of 0.05 width covering the full range of the gentrification index. I also estimate a non-linear model using separate gentrification index trinomials for the positive and negative ranges:

$$\begin{aligned}
M_{intc} = & \sum_{p=1}^3 \lambda_{Dp}(G_{nc}^p \cdot P_{nc} \cdot B_t) + \sum_{p=1}^3 \lambda_{Ap}(G_{nc}^p \cdot P_{nc}) + \lambda_B(P_{nc} \cdot B_t) + \lambda_P P_{nc} \\
& + \sum_{p=1}^3 \lambda_{Dnp}(G_{nc}^p \cdot N_{nc} \cdot B_t) + \sum_{p=1}^3 \lambda_{ANp}(G_{nc}^p \cdot N_{nc}) + \lambda_{BN}(N_{nc} \cdot B_t) + \lambda_N N_{nc} \\
& + \sigma H_{it} + d_c + \phi_{intc}
\end{aligned} \tag{15}$$

where  $p$  is the order of the polynomial term, and  $P_{nc}$  and  $N_{nc}$  indicate positive and negative values of  $G_{nc}$ , respectively. These models investigate effects for negative gentrification but also examine the importance of the precise pace of gentrification for the existence of displacement effects.

## V. Results

### A. Displacement effects

Table 3 reports the main results following estimation of equation (11).<sup>34</sup> As discussed, these estimations restrict the sample to households in neighbourhoods with non-negative gentrification index values and below-average household income per capita.<sup>35</sup> The baseline specification in column (1) reports a positive and significant coefficient for the interaction between the gentrification index and the degree days budget-shifter indicating the existence of a differential displacement effect. The coefficient size implies that poor households living a neighbourhood with a gentrification index of 0.1—a 10 percentage point higher than average increase in degree share—compared with households living a neighbourhood where gentrification is zero are 10.9 percentage points more likely to exit their housing unit per unit increase in the budget-shifter. As discussed, a one unit increase in the budget shifter is equivalent to a £100 increase in annual heating costs, equivalent to about one week of rent. Note that a 0.1 unit increase in gentrification and a £100 increase in the budget-shifter are large increases, both representing about 2 standard deviations. Nevertheless, the displacement effect seems fairly sizeable.

I consider several potential concerns regarding the exclusion restriction. In column (2), I include interactions between household characteristics and the degree days budget shifter. The differential displacement effect is largely unchanged in terms of size and significance, suggesting that this main effect is not being driven by the budget shifter impacting on the relationships between household characteristics and mobility. Further support for the idea that this channel is unimportant is that the 22 household control interactions themselves are jointly insignificant (only one of them is significant individually—at the 5% level). In column (3), I include only residents that have lived were also observed to be living in the neighbourhood in the previous year. The size of the differential displacement effect is unchanged suggesting that it is not driven by the budget shift impacting on neighbourhood composition in gentrifying neighbourhoods. The significance of the coefficient is lower, likely as a result of the smaller sample size.<sup>36</sup>

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<sup>34</sup>Table 3 reports coefficients for key variables only. Control variables are summarised in the bottom rows. A full table of coefficients is provided in Appendix A.

<sup>35</sup>I estimate effects for the full range of the gentrification index and income in later specifications.

<sup>36</sup>The private renter sample is highly mobile overall. As a result, a large share of residents are recent arrivals. However, in this specification—after excluding fresh arrivals—residents have been living in their current neighbourhood for a little over 9 years, on average. The significant differential displacement effect

In column (4), I interact the main variables with an indicator for neighbourhoods with slower rental increases.<sup>37</sup> The main coefficient is largely unchanged now that it reflects the differential displacement effect for neighbourhoods with faster rent increases, and the slow rent increase interaction is insignificant. This suggests that the main effect is not being driven by poor households' voluntary leaves being greater as a result of the budget shifter. A further alleviation of this concern is that the budget shifter variable has an insignificant direct relationship with moves in nearly all specifications.

In columns (5) and (6) I address a worry that the main result could be driven by a differential effect of degree days due to older dwelling stock in gentrifying neighbourhoods. In order to investigate this explanation, I make use of data on the share of dwelling stock in the neighbourhood that was built pre-1900 and pre-1939.<sup>38</sup> Since this dwelling stock data is not available for Scotland, I present a new baseline specification excluding households in Scotland in column (5). The main coefficient in the new baseline is roughly the same size and a little reduced in significance as a result of losing some observations compared with column (1). In column (6), I include the dwelling stock shares plus their interactions with degree days. The main effect is unchanged, and the interactions themselves are not jointly significant (nor is either individually significant), suggesting that dwelling stock is not driving the main result.

## *B. Placebo tests and robustness*

Despite having addressed the major potential threats to the exclusion restriction, one might still think that some other mechanism is at play and driving the results, or that the results are sensitive to the way the analysis has been carried out. In order to address these concerns I run a series of placebo tests and robustness analyses. As laid out below, the placebo specifications are deliberately defined to have no effect through the posited channel. Therefore, if the main results were being driven by another channel then it is likely that that effects would show up in the placebo specifications, too. The robustness checks demonstrate that main result is not sensitive to decisions made regarding data construction and estimation.

Table 4 reports results using alternative and placebo budget-shifters. The estimates using

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here shows that displacement is occurring for both newer and longer term residents.

<sup>37</sup>This indicator reflect the slowest 50% of neighbourhoods on rental increases. Rental increases are captured by the coefficient on a trend variable from a regression of reported private sector rents (BHPS) on neighbourhood constants and trends. There are slightly fewer observations in this specification since I require that there be at least 3 observations in one neighbourhood for estimating the trend.

<sup>38</sup>Neighbourhood level dwelling stock by build period is available from the Valuation Office Agency.

winter temperature, column (2), and annual temperature, column (3), as budget shifters are very similar to the baseline specification in column (1). It is reassuring that these different temperature measures, after being scaled by their different relationships with fuel bills, end up having the same effect size in the regression on household moves. The alternative measures are a little less precise, most likely due to the fact that unlike degree days, they are not specifically designed to predict heating requirements. The estimates using the placebo shifters, rainfall in columns (4) and (5), and sunshine in column (6), are in line with expectations. As discussed in the data section, it was expected that the rainfall measure may be significant, despite not predicting fuel costs, due to a negative correlation with degree days. However, this effect does not stand up to introducing the interaction with the preferred degree days budget shifter. The significance of alternative shifters and the insignificance of these placebo shifters suggest the results are not simply driven by the way the degree days measure is constructed or the nature of the times series weather data and support the idea that the effect comes through the hypothesised budget shifts.

Table 5 reports the results from placebo tests that swap the contemporaneous degree days budget shifter with different leads and lags of the same variable. The leads and lags of the budget shifter are not expected to generate displacement pressure. Future (unexpected) budget shocks will not impact on today’s vulnerability to displacement, and past budget shocks are likely to have already been adjusted for. Further, given that weather shocks are relatively random the leads and lags are unlikely to be correlated with contemporaneous degree days. Indeed, columns (1)–(5) reports that only the ‘actual’ degree days in observed period result in differential displacement effects, whereas the leads and lags are small and insignificant.

In Table 6, I make use of four sub-samples of households split by tenure and income. All four specifications are placebo tests. Social renters represent a placebo group since rents in social housing do not reflect market pressures. Social renters may be displaced by property redevelopment, in the form of estate renewal or otherwise, but this is not something that will depend on individual households budgets. Homeowners are a placebo group, too. In the UK, homeowners are not required to pay taxation on the value of their home annually, such that house price capitalisation as a result of neighbourhood gentrification cannot force a low income home-owner to move out. Columns (1)–(4), correctly find no effect for the placebo groups.

In Table 7, I demonstrate robustness across a range of different specifications. The main effects hold up when using an alternative gentrification index (a dummy for above-average

gentrification), an alternative outcome (neighbourhood exits based on observed location rather than self-reported moves), alternative estimation techniques (logit and probit), and alternative controls (no controls, year effects instead of decade dummy, and MSOA-decade fixed effects). Where comparable, the coefficients indicate similar effect sizes to the baseline specification. Since the additional year and fixed effect deliver similar but less precise estimates, I continue with the model with a simple decade dummy.

In summary, the placebo tests have shown that if something else is driving the result than it has to be something that is specific to a contemporaneous budget shift and to the private renter sample. In light of the exclusion restriction tests already carried out, it is extremely difficult to think of what this could be, other than a displacement effect. Further, the robustness checks have demonstrated that this effect is very robust across a broad range of different specifications.

### *C. Alternative specifications*

Figure 8 graphically illustrates the results of the estimation of effects by income according to equation (12) and equation (13). Both the marginal effects and the bin estimates illustrate that the differential displacement effect broadly declines with income and is insignificant for incomes above about 1.5 times the average for the city and year. However, the bin estimates make clear that the relationship with income is not continually downwards sloping. Specifically, the first bin suggest there is no significant displacement effect for households with a relative income of 0–0.5. A potential explanation for the lack of displacement effects for the lowest income group is if this group is in receipt of assistance that helps them resist displacement. However, I have already excluded from the sample those that are in receipt of housing benefits making this explanation seem less likely. A further possible explanation could be the existence of households with a low income but high wealth. These households may fall into the lowest income category without being at all vulnerable to being displaced.

In order to investigate this possibility I exclude potentially wealthy households from the sample by dropping those who report paying more in rent than they receive per year in income. Figure 9 illustrates that whilst the bottom income bin is still not significant, there is a clearer downwards slope after excluding these households. Furthermore, the increase in coefficient and improvement in significance from  $p = 0.838$  to  $p = 0.276$  for the bottom bin suggests that the explanation wealth may be important. The difference in distribution from the previous sample (the thin dashed line) indicates that these households have been dropped mainly from the bottom income bin.

Overall, the income results suggest that displacement is greater at lower incomes than at higher incomes, but that many households with an above-average income are still subject to displacement. Displacement effects are significant for households with incomes up to around 1.5 times the local average. These findings make sense within the context of the conceptual model because richer households will be more able to resist displacement, but also because richer households may be likely to belong to a group that has an upwards shift in preferences for the neighbourhood that gentrify. The model incorporates this latter explanation by assuming that group shifts in preference preserve the rank of households. However, in reality there may be some fuzziness that results in a certain extent of displacement effects all the way up the income distribution.

Figure 10 graphically illustrates the results of the estimation of non-linear effects according to equation (14) and equation (15).<sup>39</sup> Gentrification begins to result in a significant differential displacement effect at a value of around 0.1. This finding suggests that slower paced gentrification, say a 5–10 percentage points above the average increase in degree share, may occur without displacement. In the context of the model, smaller amounts of gentrification do not place so much pressure on rents thus allowing for a greater share of existing resident to resident displacement. Gentrification the occurs through the natural mobility of a neighbourhood—i.e. through excluding new poor residents rather than displacing old ones. Conversely, faster gentrification of 10 percentage or more above the average change in degree share cannot easily be absorbed by natural mobility and puts greater pressure on rents, displacing existing residents.

#### *D. Heterogeneous effects*

Table 8 examines displacement effect heterogeneity by household characteristics. Following the results of the estimation by income, I now expand the sample of private renters to include those with an income below 1.5 times the average for city and year but drop those who report earning more than they pay in rent. The new baseline in column (1) reports a slightly larger effect with this new sample. The remaining columns estimate the same model but for different sub-samples split by household characteristics. The findings imply that the effect size is reasonably constant across age, gender and education attainment of the head of household, as well as whether children live in the household. The apparent lack of effect

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<sup>39</sup>The non-linear models use a larger sample of households since those living in neighbourhoods with negative gentrification are also included. Furthermore, I now include all households with a relative income less than 1.5 (rather than less than 1 as in the baseline) following the results of the income models.

heterogeneity may conceal differential responses. For example, households with children may be more sensitive to budget shocks but this may be cancelled out by greater costs to moving. The results by educational attainment are reassuring to the extent that the main result is not driven purely by household heads with a degree, which could indicate something mechanical.

Table 9 examines displacement effect heterogeneity by neighbourhood type and location. Interestingly, the characterisation of gentrification as something that happens to working-class urban neighbourhoods receives support in terms of the findings on displacement. Only urban neighbourhoods have significant displacement effects (although the coefficients are similar) perhaps reflecting the fact that gentrification may exert more pressure where there is a tighter housing supply. The displacement effect appears to be coming through working-class neighbourhoods—a below-average degree share at the start of the intercensal period—rather than middle-class neighbourhoods. This could be explained if rent increases are more sensitive to changes in demographics in the early stages than when the neighbourhood is already gentrified. Finally, the displacement effect appears present for England and Scotland but not for Wales—perhaps a combinations of the fact that Wales is very rural with the fact that the sample size is small.

## VI. Conclusion

I have provided causal evidence on the existence of displacement of low-income private renters. I have done so by constructing an index of gentrification and estimating the differential effect with exogenous budget shifter on household moves. The main finding is robust to inclusion of controls for threats to the exclusion restriction. Estimating the model using placebo groups and placebo treatments finds no effect, which supports the validity of main result.

Existing evidence suggested that gentrification occurs without displacement. The difference in findings could represent a number of factors. Most of the existing evidence is based on the U.S., which is a different context compared with the UK. Furthermore, previous studies have typically examined representative samples of households that include both homeowners and renters, of all income groups. I examine the poorest half of private renters, which is a particularly low-income group in the UK. Finally, I have developed and implemented an empirical approach that aims to provide a causal estimate of the displacement effect, whereas estimates from previous studies may be biased by the endogeneity between gentrification and mobility.

The empirical approach itself and the differential treatment effect (DATE) it reveals may



have more general applications. It may be a useful approach in evaluating phenomenon that have interactive treatment effects but where it is difficult to find a valid instrument or threshold/kink.

If my causal estimates are to be believed then they challenge the consensus regarding displacement, and suggest the need to rethink gentrification and its consequences. Place-based policies of the urban renewal type, that aim to encourage mixed communities, may not be justified on the grounds that they are beneficial to existing residents, especially if many of those existing residents rent privately. If policymakers wish to improve outcomes for low-income residents that rent privately it may be more effective to instead provide assistance with resisting displacement pressures resulting from market- and intervention-led gentrification.

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Figure 4: Gentrification index for London, 1990s

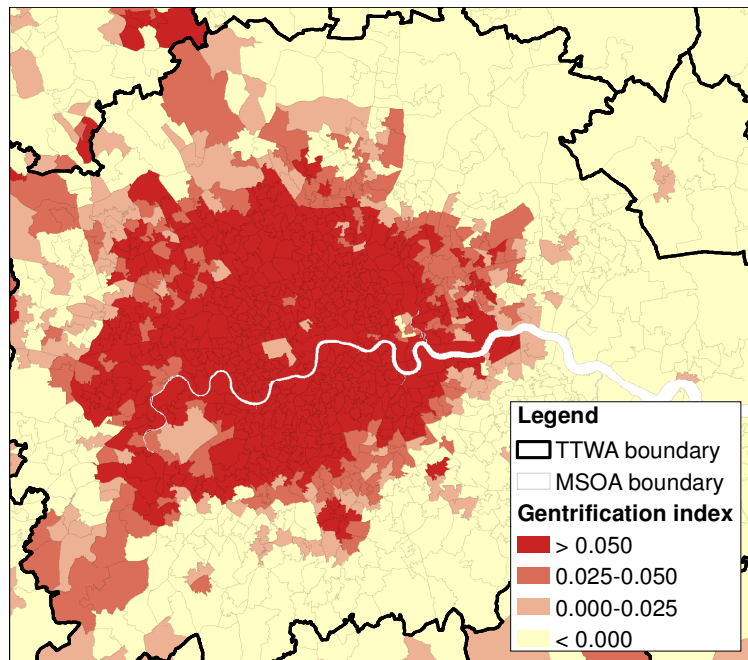


Figure 5: Gentrification index for London, 2000s

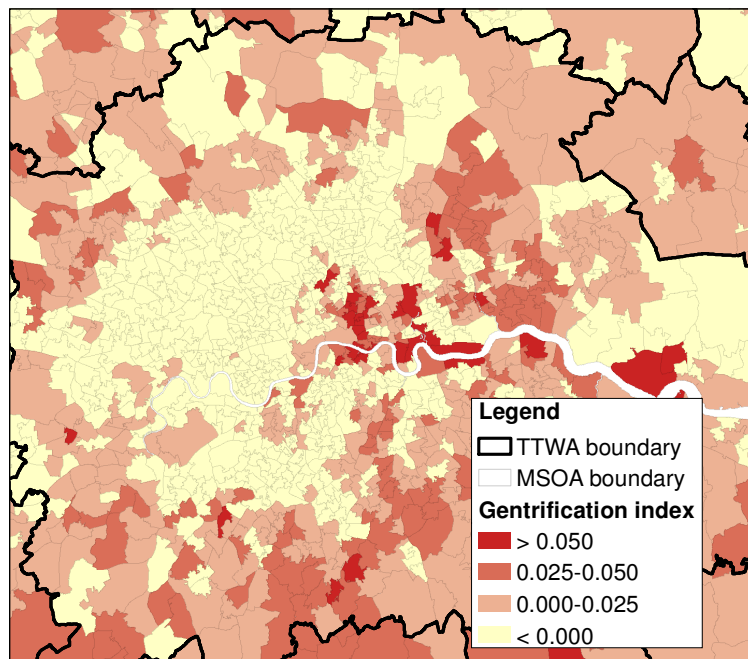
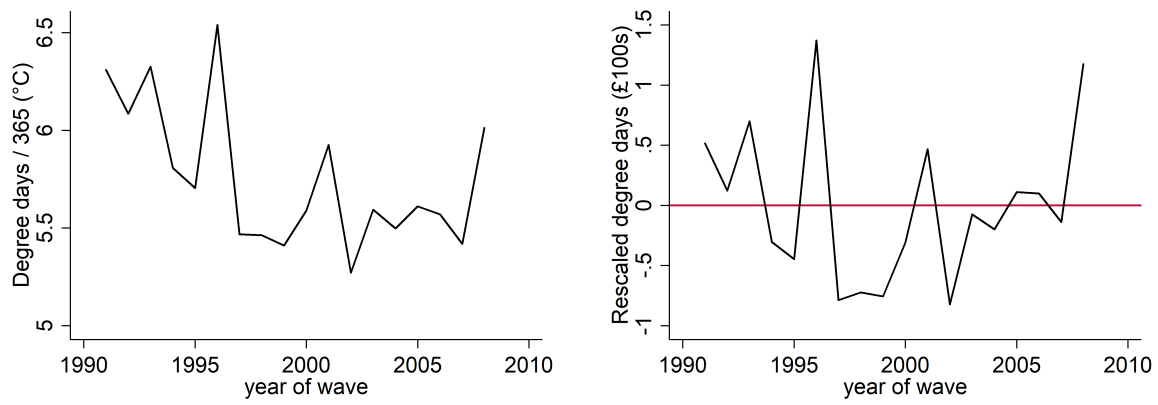
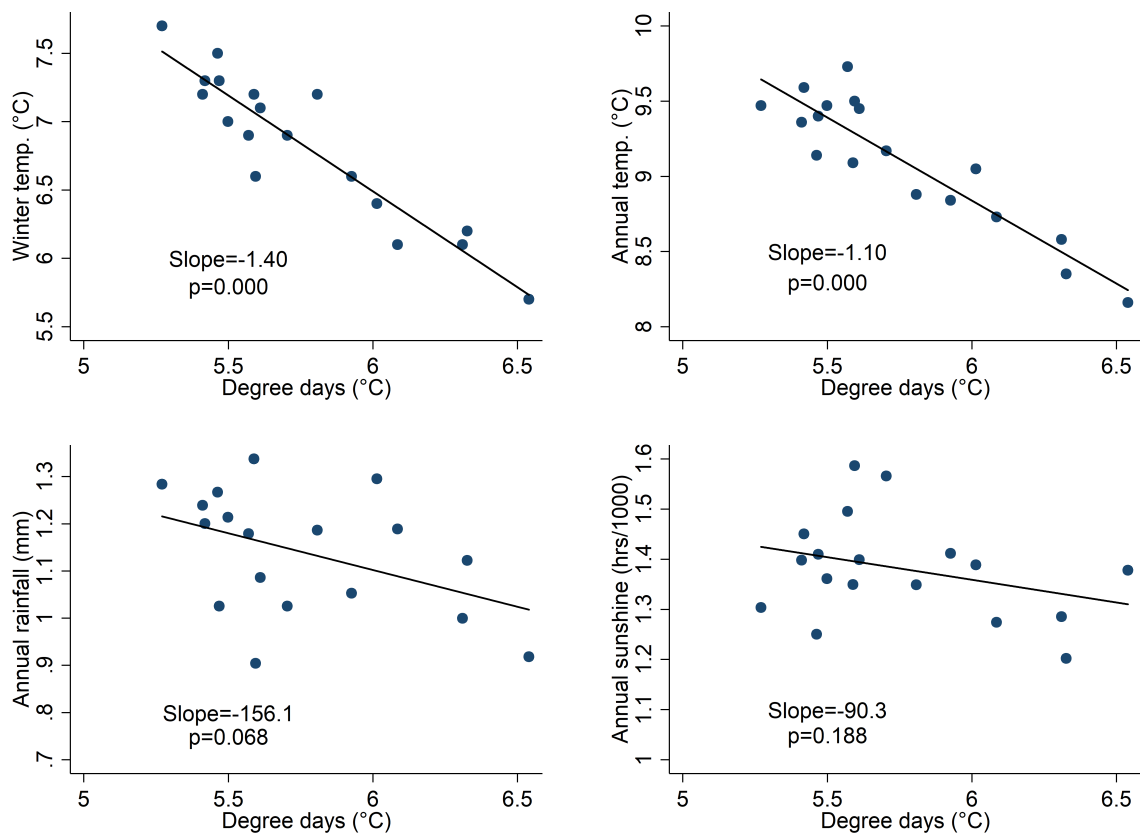


Figure 6: Degree days budget-shifter



*Notes:* The right plot is the raw degree days divided by 365. The left plot is the degree days rescaled by its impact on fuel bills.

Figure 7: Alternative and placebo budget-shifter correlations with degree days



*Notes:* Scatter plots and lines of best fit for regressions of alternative and placebo budget shifters on the preferred budget shifter, degree days. The alternative shifters are winter temperature and annual temperature, and the placebo shifters are rainfall and sunshine.

Figure 8: Displacement effect by income

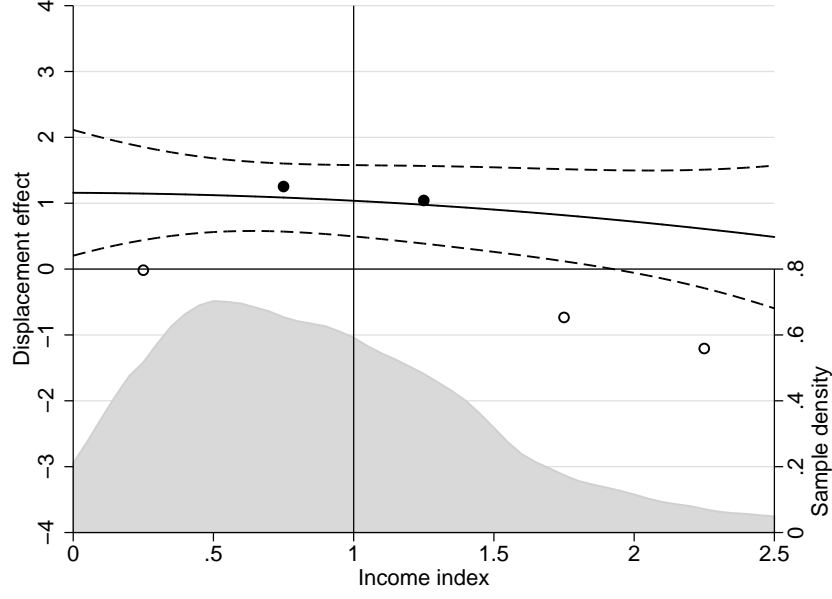
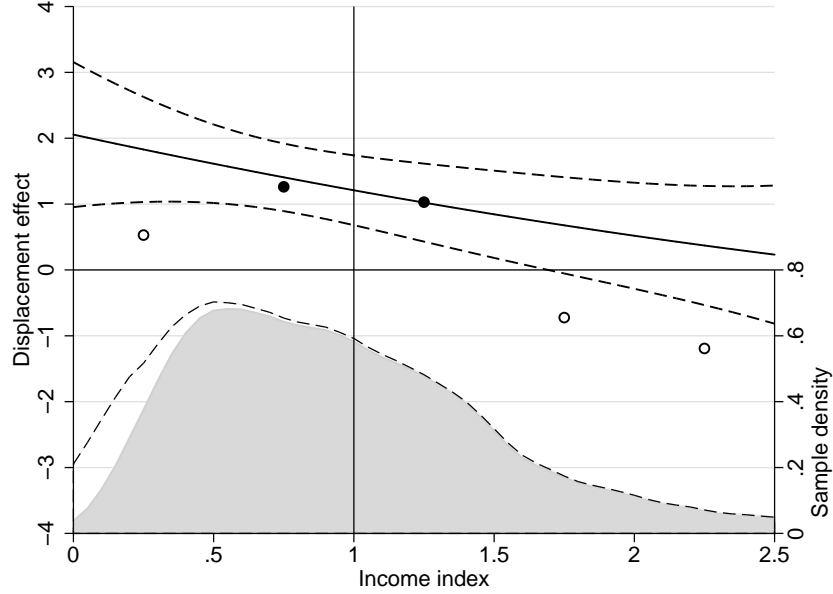


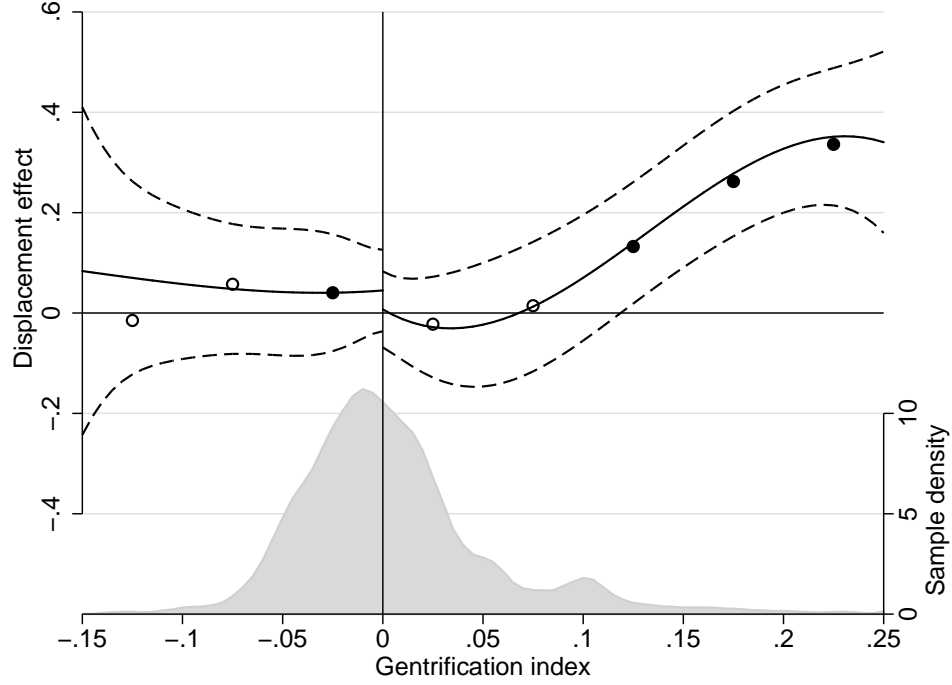
Figure 9: Displacement effect by income excluding potentially wealthy low income households



*Notes:* Graphical illustrations of the results of the estimation equation (12) and equation (13). The solid line plots the marginal displacement effect evaluated at different levels of household income (based on a binomial) as  $\lambda_D G_{nc} + \lambda_{D1}(G_{nc} \cdot I_{it}) + \lambda_{D2}(G_{nc} \cdot I_{it}^2)$ . The dashed lines represent the 10% confidence intervals reflecting meaningful standard errors computed for marginal effects following [Aiken and West \[1991\]](#). The markers plot bin estimates of the differential displacement effect in income bin  $h$ , i.e. the parameters  $\lambda_{Dh}$ . The filled markers are significant at the 10% level whereas the empty markers are insignificant. The grey area represents a kernel density plot of the sample distribution. The thin dashed line in the bottom figure plots the sample distribution prior to excluding potentially wealthy households.



Figure 10: Non-linear displacement effect



*Notes:* Graphical illustration of the results of the estimation equation (14) and equation (15). The solid line plots the marginal differential displacement effect evaluated as  $\sum_{p=1}^3 \lambda_{Dp} G_{nc}^p + \lambda_P$  at positive gentrification values and as  $\sum_{p=1}^3 \lambda_{ANp} G_{nc}^p + \lambda_N$  at negative gentrification values. The dashed lines represent the 10% confidence intervals reflecting meaningful standard errors computed for marginal effects following [Aiken and West \[1991\]](#). The markers plot bin estimates of the differential displacement effect for each gentrification bin  $b$ , i.e. the parameters  $\lambda_{Db}$ . The filled markers are significant at the 10% level whereas the empty markers are insignificant. The grey area represents a kernel density plot of the sample distribution.

Table 1: Summary statistics for private renters

Variable	N	Mean	SD	Min	Max
If head moves house	5362	0.46	0.50	0.00	1.00
Gentrification index	5361	0.01	0.05	-0.21	0.27
Household income per cap. (city & year adj.)	5362	0.92	0.71	0.00	11.57
Age of head of household	5362	35.55	16.55	16.00	93.00
Age of head of household squared	5362	1537.60	1577.34	256.00	8649.00
If head of household holds a university degree	5362	0.22	0.42	0.00	1.00
Housing benefit share is not reported	5362	0.03	0.18	0.00	1.00
Number of children in household	5362	0.32	0.75	0.00	7.00
Number of people per room in house	5362	0.61	0.32	0.00	5.00
People per room is not reported	5362	0.01	0.08	0.00	1.00
If head of household is male	5362	0.50	0.50	0.00	1.00
If head of household pensionable age (>65)	5362	0.08	0.27	0.00	1.00
If economic status is self-employed	5362	0.07	0.26	0.00	1.00
If economic status is employed	5362	0.56	0.50	0.00	1.00
If economic status is unemployed	5362	0.05	0.22	0.00	1.00
If born outside of UK	5362	0.01	0.09	0.00	1.00
If marital status is married	5362	0.18	0.38	0.00	1.00
If marital status is divorced	5362	0.18	0.38	0.00	1.00
If marital status is widowed	5362	0.05	0.21	0.00	1.00
Number of years living at address	5362	3.42	9.55	0.00	70.00
Years living at address is unknown	5362	0.09	0.28	0.00	1.00
If likes neighbourood	5362	0.90	0.30	0.00	1.00
Satisfaction with house (Score 0-7)	5362	3.35	2.51	0.00	7.00
Satisfaction with house is unknown	5362	0.29	0.46	0.00	1.00

*Notes:* Summary statistics for private renter households from the BHPS. Private renters in receipt of housing benefits have been excluded. Reference categories are inactive for economic status and single for marital status. Household income is divided by the number of household members and then regressed on city (travel-to-work area) and year fixed effects. The relative income measure is then computed as income per capita divided by the income predicted by the year and city effects.

Table 2: Fuel bill effects of real and placebo budget-shifters

Dependent variable: annual fuel bill (£100s)	Degree days °C (1)	Winter temp. °C (2)	Annual temp. °C (3)	Annual rainfall mm/1000 (4)	Annual sunshine hours/1000 (5)
Budget-shifter	2.085*** (0.530)	−1.658*** (0.536)	−0.896*** (0.329)	0.739 (1.182)	−1.265 (1.684)
Constant	7.251*** (0.136)	7.234*** (0.138)	7.227*** (0.135)	7.179*** (0.135)	7.183*** (0.135)
Observations	1485	1485	1485	1485	1485

*Notes:* OLS estimations of fuel bills on detrended and demeaned budget-shifters using sample of low-income private renters that report fuel bill. Degree days is the preferred budget-shifter. Winter temperature and annual temperature are alternative budget shifters. Rainfall and sunshine are placebo budget shifters. The low-income sample is below-average on household income per capita relative to year and city average. Compared with the main analysis, this regression is able to use heads of household who are not observed again in the next period (making the sample larger) but not able to use households that do not report fuel bills (making the sample smaller). Robust standard errors are in parentheses.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Displacement effects for low income private renters

Dependent variable: if moves house	Great Britain sample				England & Wales sample	
	Baseline specification	Household type interactions	Excluding new residents	Slow rents interaction	Baseline specification	Dwelling stock interactions
	(1)	(2)	(3)	(4)	(5)	(6)
Gentrification $\times$ Degree days	1.091*** (0.375)	0.964** (0.397)	1.009* (0.600)	1.030** (0.506)	0.980* (0.504)	1.034* (0.531)
Gentrification	0.305 (0.270)	0.331 (0.275)	-0.073 (0.430)	0.532 (0.353)	0.077 (0.431)	0.185 (0.432)
Degree days	-0.041 (0.029)	-0.254 (0.173)	-0.022 (0.045)	-0.029 (0.041)	-0.025 (0.032)	-0.078 (0.063)
Gentrification $\times$ Degree days $\times$ Slow rents				0.008 (0.973)		
Gentrification $\times$ Slow rents				-0.747 (0.463)		
Degree days $\times$ Slow rents				0.009 (0.064)		
Decade dummy (1 var.)	YES	YES	YES	YES	YES	YES
Wald test p-value	0.010	0.013	0.050	0.010	0.045	0.053
Household controls (22 vars.)	YES	YES	YES	YES	YES	YES
Wald test p-value	0.000	0.000	0.000	0.000	0.000	0.000
Household controls (22 vars.) $\times$ Degree days	.	YES	.	.	.	.
Wald test p-value	.	0.201	.	.	.	.
Dwelling stock (2 vars.)	.	.	.	.	.	YES
Wald test p-value	.	.	.	.	.	0.376
Dwelling stock (2 vars.) $\times$ Degree days	.	.	.	.	.	YES
Wald test p-value	.	.	.	.	.	0.323
Observations	1527	1527	668	1141	1171	1171

*Notes:* Ordinary least squares estimation of equation (11) using sample of low-income private renters. The low-income sample is below-average on household income per capita relative to year and city average. Control variables are summarised in the bottom rows. The regression also includes a constant. The full table of regression coefficients is reported in Appendix A. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4: Alternative and placebo budget-shifters

Dependent variable: if moves house	Degree days (1)	Winter temp. (2)	Annual temp. (3)	Rainfall (4)	Rainfall (5)	Sunshine (6)
Gentrification $\times$ Budget-shifter	1.098*** (0.375)	1.042* (0.555)	0.954** (0.470)	-3.589* (2.077)	-2.006 (2.316)	-1.247 (1.345)
Gentrification $\times$ Degree days					0.979** (0.414)	
Observations	1529	1529	1529	1529	1529	1529

*Notes:* Estimations of equation (11) with alternative and placebo budget shifters using sample of low-income private renters. The low-income sample is below-average on household income per capita relative to year and city average. Columns (1) and (2) are alternative budget-shifters. Columns (3), (4) and (5) are placebo shifters, where rainfall is expected to work as a placebo only in column (4) since it is correlated with degree days (see discussion in data section). Also included in the regression are gentrification (uninteracted), degree days (uninteracted), 22 household control variables, a decade dummy and a constant. Standard errors in parentheses are clustered on MSOA-decades in all models.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 5: Placebo test: leads and lags of degree days

Dependent variable: if moves house	Lag 2 (1)	Lag 1 (2)	Actual (3)	Lead 1 (4)	Lead 2 (5)
Gentrification $\times$ Degree days (Lag/Lead)	0.313 (0.261)	-0.305 (0.283)	1.091*** (0.375)	-0.004 (0.238)	0.287 (0.228)
Observations	1403	1469	1527	1527	1420

*Notes:* OLS estimations of equation (11) for sample of low-income private renters using leads and lags of budget shifter as a placebo treatment. The low-income sample is below-average on household income per capita relative to year and city average. Also included in the regression are gentrification (uninteracted), degree days (uninteracted), 22 household control variables, a decade dummy and a constant. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 6: Placebo tests: other tenure types

Dependent variable: if moves house	Social		Homeowner	
	Poor (1)	Rich (2)	Poor (3)	Rich (4)
Gentrification $\times$ Degree days	-0.136 (0.196)	-0.269 (0.216)	-0.125 (0.104)	0.118 (0.113)
Observations	4796	937	14717	13578

*Notes:* OLS estimations of equation (11) for different tenure-income groups. To remain consistent, the average income for the private renter sample is used to define rich and poor groups for other tenure types. The income variable used is household income per capita relative to year and city average. Also included in the regression are gentrification (uninteracted), degree days (uninteracted), 22 household control variables, a decade dummy and a constant. Standard errors in parentheses are clustered on MSOA-decades. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 7: Robustness

Dependent variable: if moves house	Baseline (1)	Gentrification dummy (2)	Outcome var.: exit neigh. (3)	Probit (4)	Logit (5)	No controls (6)	Fixed effects & year effects (7)	MSOA-decade & year effects (8)
Gentrification $\times$ Degree days	1.091*** (0.375)	0.094** (0.039)	1.124*** (0.386)	3.620*** (1.345)	6.419*** (2.307)	1.084** (0.453)	1.474** (0.663)	1.598** (0.678)
Gentrification	0.305 (0.270)	0.010 (0.025)	0.256 (0.270)	1.146 (0.880)	2.029 (1.474)	1.164*** (0.383)	0.891 (0.925)	
Degree days	-0.041 (0.029)	-0.027 (0.025)	-0.052* (0.029)	-0.133 (0.096)	-0.245 (0.164)	-0.072** (0.032)		
Decade dummy (1 var.)	YES	YES	YES	YES	YES	.	.	.
Household controls (22 vars.)	YES	YES	YES	YES	YES	.	YES	YES
Year dummies (16 vars.)	.	.	.	.	.	.	YES	YES
MSOA fixed effects	.	.	.	.	.	.	YES	.
MSOA-decade fixed effects	.	.	.	.	.	.	.	YES
Observations	1527	1527	1527	1527	1527	1527	1527	1527

*Notes:* Robustness checks for the estimation of equation (11) using sample of low-income private renters. The low-income sample is below-average on household income per capita relative to year and city average. Col. (2) uses a gentrification dummy that indicates above-average gentrification for the positive values sample. Col. (3) uses the alternative outcome of neighbourhood exits based on observed location rather than self-reported moves. Col. (4) estimates the model using logit. Col. (5) estimates the model using probit. Col. (6) estimates the model without any control variables. Col. (7) estimates the model with individual year effects instead of a decade dummy. Col. (8) estimates the model with MSOA-decade fixed effects instead of just MSOA fixed effects. The regressions also include a constant. Standard errors in parentheses are clustered on MSOA-decades in all models.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 8: Effect heterogeneity: household

Dependent variable: if moves house	Baseline (1)	Head younger than 28 years (2)	Head 28 years or older (3)	Head is male (4)	Head is female (5)	Children in household (6)	Head has uni. degree (7)	Head has no uni. degree (8)
Gentrification $\times$ Degree days	1.385*** (0.330)	1.578*** (0.511)	1.433*** (0.415)	1.573*** (0.512)	1.216*** (0.457)	1.808** (0.747)	1.753*** (0.546)	1.474*** (0.458)
Gentrification	0.501* (0.265)	1.076*** (0.346)	0.172 (0.342)	0.973** (0.391)	0.329 (0.325)	0.745 (0.652)	0.635 (0.409)	0.572* (0.333)
Degree days	-0.048* (0.028)	-0.060 (0.044)	-0.054 (0.036)	-0.077* (0.041)	-0.028 (0.039)	-0.147** (0.068)	-0.096 (0.059)	-0.045 (0.033)
Observations	1891	834	1057	919	972	365	457	1434

*Notes:* OLS estimations of equation (11) using samples of low-income private renters split by household characteristics. The low-income sample is below 1.5 times average household income per capita for the year and city. Also included in the regression are gentrification (uninteracted), degree days (uninteracted), 22 household control variables, a decade dummy and a constant. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 9: Effect heterogeneity: neighbourhood

Dependent variable: if moves house	Baseline (1)	Urban (2)	Rural (3)	Working class (4)	Middle class (5)	England (6)	Wales (7)	Scotland (8)
Gentrification $\times$ Degree days	1.385*** (0.330)	1.470*** (0.364)	1.262 (1.625)	2.442*** (0.442)	0.654 (0.418)	1.368*** (0.479)	-3.168 (6.772)	2.152*** (0.632)
Gentrification	0.501* (0.265)	0.405 (0.286)	0.769 (1.059)	0.417 (0.395)	0.631* (0.335)	0.419 (0.413)	5.362** (2.025)	0.920* (0.467)
Degree days	-0.048* (0.028)	-0.066* (0.034)	-0.003 (0.056)	-0.122*** (0.043)	0.015 (0.035)	-0.045 (0.036)	0.068 (0.088)	-0.144* (0.080)
Observations	1891	1359	532	721	1170	1299	202	390

*Notes:* OLS estimations of equation (11) using samples of low-income private renters split by neighbourhood type/location. The low-income sample is below 1.5 times average household income per capita for the year and city. Also included in the regression are gentrification (uninteracted), degree days (uninteracted), 22 household control variables, a decade dummy and a constant. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



# Appendix A.

Table A1: Full version of main table

	Baseline GB specification (1)	Household type interactions (2)	Excluding new residents (3)	Slow rents interaction (4)	Baseline E&W specification (5)	Dwelling stock interactions (6)
Gentrification $\times$ Degree days	1.091*** (0.375)	0.964** (0.397)	1.009* (0.600)	1.030** (0.506)	0.980* (0.504)	1.034* (0.531)
Gentrification	0.305 (0.270)	0.331 (0.275)	-0.073 (0.430)	0.532 (0.353)	0.077 (0.431)	0.185 (0.432)
Degree days	-0.041 (0.029)	-0.254 (0.173)	-0.022 (0.045)	-0.029 (0.041)	-0.025 (0.032)	-0.078 (0.063)
Household income per cap. (city & year adj.)	-0.106** (0.047)	-0.090* (0.047)	-0.205** (0.080)	-0.174*** (0.055)	-0.107* (0.055)	-0.110** (0.055)
Age of head of household	-0.027*** (0.005)	-0.027*** (0.005)	-0.028*** (0.006)	-0.030*** (0.005)	-0.029*** (0.006)	-0.028*** (0.006)
Age of head of household squared	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
If head of household holds a university degree	0.089*** (0.034)	0.089** (0.036)	0.083* (0.050)	0.075* (0.039)	0.081** (0.040)	0.080** (0.040)
Housing benefit share is not reported	-0.047 (0.070)	-0.067 (0.070)	0.062 (0.155)	-0.117 (0.080)	-0.042 (0.103)	-0.028 (0.104)
Number of children in household	-0.060*** (0.015)	-0.061*** (0.015)	-0.050*** (0.019)	-0.078*** (0.018)	-0.066*** (0.017)	-0.066*** (0.017)
Number of people per room in house	0.093*** (0.029)	0.093*** (0.035)	0.144*** (0.051)	0.125*** (0.042)	0.111*** (0.035)	0.109*** (0.035)
People per room is not reported	0.050 (0.094)	0.076 (0.091)	0.013 (0.145)	0.026 (0.097)	0.024 (0.094)	0.015 (0.097)
If head of household is male	0.007 (0.026)	0.003 (0.027)	0.014 (0.038)	-0.001 (0.030)	-0.011 (0.030)	-0.011 (0.030)
If head of household pensionable age (>65)	-0.058 (0.070)	-0.074 (0.068)	0.016 (0.094)	-0.040 (0.068)	-0.050 (0.079)	-0.049 (0.079)
If economic status is self-employed	-0.063 (0.052)	-0.063 (0.053)	-0.077 (0.065)	-0.036 (0.054)	-0.022 (0.059)	-0.021 (0.060)
If economic status is employed	-0.043 (0.031)	-0.047 (0.032)	-0.058 (0.048)	-0.021 (0.036)	-0.057 (0.036)	-0.056 (0.036)
If economic status is unemployed	-0.124** (0.050)	-0.104** (0.050)	-0.136** (0.067)	-0.084 (0.058)	-0.090 (0.060)	-0.091 (0.060)
If born outside of UK	-0.111	-0.110	-0.064	-0.059	-0.227***	-0.221**

Continued on next page

Table A1: Full version of main table (continued from previous page)

	(1)	(2)	(3)	(4)	(5)	(6)
	(0.084)	(0.081)	(0.128)	(0.089)	(0.087)	(0.088)
If marital status is married	0.019	0.009	0.094*	0.070	0.012	0.009
	(0.044)	(0.045)	(0.055)	(0.046)	(0.050)	(0.050)
If marital status is divorced	0.074	0.067	0.121**	0.099**	0.061	0.055
	(0.046)	(0.046)	(0.058)	(0.049)	(0.054)	(0.054)
If marital status is widowed	0.058	0.059	0.059	0.051	0.014	-0.005
	(0.066)	(0.067)	(0.067)	(0.072)	(0.068)	(0.068)
Number of years living at address	-0.001	-0.002	0.002	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years living at address is unknown	-0.102**	-0.080*	0.000	-0.071*	-0.102**	-0.105**
	(0.040)	(0.041)	(.)	(0.040)	(0.050)	(0.050)
If likes neighbourood	-0.090**	-0.091**	-0.050	-0.070*	-0.072*	-0.075*
	(0.036)	(0.037)	(0.060)	(0.042)	(0.040)	(0.040)
Satisfaction with house (Score 0-7)	-0.040***	-0.036***	-0.048***	-0.037***	-0.032***	-0.032***
	(0.009)	(0.011)	(0.014)	(0.011)	(0.011)	(0.011)
Satisfaction with house is unknown	-0.265***	-0.247***	-0.359***	-0.274***	-0.207***	-0.202***
	(0.054)	(0.062)	(0.082)	(0.062)	(0.061)	(0.060)
Decade is 2000s	-0.071**	-0.069**	-0.077**	-0.084***	-0.064**	-0.065*
	(0.028)	(0.028)	(0.039)	(0.032)	(0.032)	(0.034)
Household income $\times$ Degree days		0.092				
		(0.085)				
Age of head $\times$ Degree days		0.003				
		(0.008)				
Age of head squared $\times$ Degree days		0.000				
		(0.000)				
If head has degree $\times$ Degree days		0.066				
		(0.053)				
Housing benefit not reported $\times$ Degree days		-0.066				
		(0.105)				
No. children in household $\times$ Degree days		-0.004				
		(0.034)				
People per room in house $\times$ Degree days		0.012				
		(0.067)				
People per room not reported $\times$ Degree days		0.090				
		(0.182)				
If head of household is male $\times$ Degree days		-0.012				
		(0.042)				
If head is pensioner (>65yo) $\times$ Degree days		-0.171				
		(0.133)				

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Continued on next page

Table A1: Full version of main table (continued from previous page)						
	(1)	(2)	(3)	(4)	(5)	(6)
	(0.103)	(0.110)	(0.152)	(0.121)	(0.118)	(0.120)
Observations	1527	1527	668	1141	1171	1171

*Notes:* Ordinary least squares estimation of equation (11) using sample of low income private renters. The low income sample is below-average on household income per capita relative to year and city average. Standard errors in parentheses are clustered on MSOA-decades.

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

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